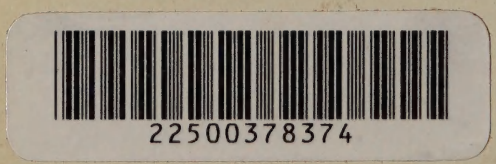
85.


22500378374

Presented to the Library
by
Ernest Hartley

Archives of Medicine

A BI-MONTHLY JOURNAL

DEVOTED TO ORIGINAL COMMUNICATIONS ON MEDICINE, SURGERY, AND THEIR SPECIAL BRANCHES

EDITED BY

E. C. SEGUIN, M.D.

MEDICAL LIBRARY. ASSOCIATION

S'il est possible de perfectionner l'espèce humaine, c'est dans la médecine qu'il faut en chercher les moyens.

—DESCARTES

FOURTH VOLUME

NEW YORK

COPYRIGHT BY

G. P. PUTNAM'S SONS, 182 FIFTH AVENUE

LONDON:—WILLIAMS & NORGATE, 14 HENRIETTA STREET, COVENT GARDEN

1880

COPYRIGHT BY
G. P. PUTNAM'S SONS.
1880

WELLCOME INSTITUTE LIBRARY
Coll. WelMOmec
Coll. No.

Press of
G. P. Putnam's Sons
182 *Fifth Avenue*

INDEX.

Acute Pernicious Anæmia with Recovery 96
Alt, Adolph. Lectures on the Human Eye in its Normal and Pathological Conditions. Review 212
Anæmia ; Treatment by Cold Pack and Massage . . 51, 163
Antiseptic Surgery. Review . 294
Auscultatory Percussion . . 270

Beard, Geo. M. A Practical Treatise on Nervous Exhaustion. Review 88
BERMANN, I. The Fungus of Syphilis 263

Case of Labor with Avulsion of the Uterus. Recovery . . . 93
Chronic Rheumatism, Practical Remarks on 120
Cold Pack in the Treatmemt of Anæmia 51, 163
Coloboma of Eyelids 44
Congenital Coloboma of the Eyelids 44
Contributions to Orthopædic Surgery. Review 197
Cultivation of Specialties in Medicine 280

Dowse, T. S. Neuralgia ; its Nature and Treatment. Review 91

Epilepsy, Thalamic 1

ERB, WILHELM. Spinal Myosis and Reflex Pupillary Immobility 105

Fothergill, J. Milner. The Practitioner's Hand-book of Treatment. Review 302
Fox, George Henry. Photographic Illustrations of Skin Diseases. Review 295
Fracture of the Patella. Review 202
Friedrichshall Bitterwater, Physiological Effects of 97
Fungus of Syphilis 263

Genealogical Table of Family with Muscular Atrophy. Osler's 319
——— Naunyn's 320
GRAY, LANDON CARTER. The Use of Quinine with Nervous Sedatives 191
Gross, S. W. A Practical Treatise on Tumors of the Mammary Gland. Review . . . 313
GRUENING, E. Congenital Coloboma of the Eyelids 44

Hamilton, Frank H. Fracture of the Patella. Review . . 202
HAMMOND, W. A. On Thalamic Epilepsy 1
HEINEMAN, H. N. Indican in Health and Disease 24
Heredity in Progressive Muscular Atrophy 316

Hutchison, Joseph C. Contributions to Orthopædic Surgery. Review 197

Indican in Health and Disease . 24
Insolation, Case of 215

Jacobi, Mary Putnam and White, Victoria A. On the use of the Cold Pack followed by Massage in the Treatment of Anæmia 51, 163

Lectures on the Human Eye in its Normal and Pathological Conditions. Review 212
Legg, J. Wickham. On the Bile, Jaundice, and Bilious Diseases. Review 308
Lücke. Surgical Diagnosis of Tumors Review 294
Lunacy Reform. No. iv, . . . 73

McBride, T. A. Auscultatory Percussion 279
McCormac, W. Antiseptic Surgery. Review 297
Massage in the treatment of Anæmia 51, 163
Mundè, Paul F. The Diagnosis and Treatment of Obscure Pelvic Abscess in Women, etc. 234
Mysophobia, Case of 102

Narcolepsy 94
Neuralgia ; its Nature and Treatment. Review 91
Night-sweating in Phthisis . . 95

Occipital Headache as a Symptom of Uræmia 98
On the use of the Cold Pack followed by Massage in the Treatment of Anæmia . . . 51, 163
On the Bile, Jaundice, and Bilious Diseases. Review . . 308
Osler, William. On Heredity in Progressive Muscular Atrophy as illustrated in the Farr family of Vermont 316

Paræsthesia of the Hands . . 147
Pathology of Night-sweating in Phthisis 95
Pelvic Peritonitis and Pelvic Cellulitis, Diagnosis of 234
Pepper, William. Some Practical Remarks on Chronic Rheumatism 120
Photographic Illustrations of Skin Diseases. Review 295
Practical Treatise on Tumors of Mammary Gland. Review . 313
Practical Treatise on Nervous Exhaustion. Review . . . 88
Practitioners' Hand-book of Treatment. Review . . . 302
Progressive Muscular Atrophy . 316
Psycho-physiological Training of an Idiotic Eye 217
Putnam, James J. A Series of cases of Paræsthesia, mainly of the Hands, of Periodical Recurrence, and possibly of Vaso-motor Origin 147

Right of the Insane to Liberty . 73

Seguin, Edward. Psycho-physiological Training of an Idiotic Eye 217
Seguin, E. C. The Right of the Insane to Liberty 73
——— On Occipital Headache as a Symptom of Uræmia . . 98
——— Case of Mysophobia . . 102
——— The Cultivation of Specialties in Medicine 280
Series of cases of Paræsthesia, mainly of the Hands . . . 147
Specialties in Medicine . . . 280
Spinal Myosis and Reflex Pupillary Immobility 105
Strychnia and other Remedies for Night-sweating in Phthisis . . 95
Students' Manual of Venereal Diseases. Review 86
Sturgis F. R. The Student's Manual of Venereal diseases. Review 86

Surgical Diagnosis of Tumors. Review . . . . . , 294
Syphilis, the Fungus of . . . 263

Thalamic Epilepsy 1
TOWNSEND F. W. Case of Insolation 215
Transactions of the American Gynecological Society. Review 83
Use of Quinine with Nervous Sedatives 191

Water as a Prophylactic and a Remedy 38
WEBBER S. G. Water as a Prophylactic and a Remedy . . 38
WHITE, VICTORIA A. and JACOBI, MARY P. On the use of the Cold Pack in Anæmia, etc. 51, 163

VOL. IV. No. 1. AUGUST, 1880.

ARCHIVES OF MEDICINE.

Original Articles.

ON THALAMIC EPILEPSY.*

By WILLIAM A. HAMMOND, M.D.,

SURGEON-GENERAL U. S. ARMY (RETIRED); PROFESSOR OF DISEASES OF THE MIND AND NERVOUS SYSTEM IN THE UNIVERSITY OF NEW YORK, ETC.

WHILST cases of the form of epilepsy I am about to describe have doubtless been not very uncommon, it has happened that they have not hitherto received special attention. Either they have been regarded as something entirely different from epilepsy, or the characteristic phenomena have been overlooked or merged into a general description with little stress being laid upon them. So far as I know, therefore, this is the first attempt to differentiate them and to associate the symptoms by which they are chiefly marked with a definite brain-lesion.

In an interesting paper Dr. Hughlings Jackson † specifies the following six varieties of epilepsy, as embracing all known forms of the disease:

1. A sudden and temporary stench in the nose with transient unconsciousness.

2. A sudden and temporary development of blue vision.

* Read before the American Neurological Association, June 18, 1880.

† "On the Anatomical, Physiological and Pathological Investigation of Epilepsies," *West Riding Lunatic Asylum Medical Reports*, Vol. iii, 1873, p. 334.

3. A spasm of the right side of the face with stoppage of speech.

4. A tingling of the index finger and thumb, followed by spasm of the hand and forearm.

5. A convulsion almost instantly universal with immediate loss of consciousness.

6. Certain vertiginous attacks.

As is seen, Dr. Jackson does not make loss of consciousness an essential feature of the epileptic paroxysm, and this fact is apparent from his reiterated assertions, not only throughout the paper in question, but in other contributions which he has made to our knowledge of epilepsy. He defines the disease as "An occasional, sudden and rapid discharge of gray matter of some part of the brain,"† a bad definition certainly, but I do not quote it now to criticise it further than to call attention to the fact that the element of unconsciousness is not included in its terms.

The more familiar I become with epileptic and epileptiform phenomena, the more convinced I am that there is no true epilepsy without unconsciousness. If we take out this condition, we have a hundred or more trifling symptoms occurring daily in individuals in good health, which never pass into any more highly developed state, any one of which according to Dr. Hughlings Jackson, constitutes a disease which all mankind regard as of terrible import. The tendency of all epilepsies of mild type is to pass into others of more severe character, and some of the forms mentioned by Dr. Jackson never evince the slightest disposition toward this advance.

Thus, I have repeatedly had patients under my charge, who had at times been the subject of tingling of the index finger, the thumb and other fingers, followed by spasmodic movements of certain muscles of the forearm and arm, and who, not having unconsciousness, I have not considered af-

‡ Op. cit., p. 315, note, *et seq.*, p. 331.

fected with epilepsy, and in whom cures were effected without there being any more severe epileptoid symptoms. I should be very sorry to regard every patient I have seen, who had occasionally had "a sudden and temporary development of blue vision," as being the victim of epilepsy—for I have seen many such in whom there was no reason to suspect the existence of this terrible malady, and in whom such a symptom was without other apparent significance than as indicating a disordered stomach. When this was remedied, the blue visional paroxysms disappeared without further treatment, often to return when a like cause was brought into action.

In all epilepsies I should regard unconsciousness as the essential phenomenon, without which, in fact, there is no epilepsy. The other symptoms are the characteristic features, those which constitute the differentiations, vertigo, convulsions, sensory disturbances, mania and other psychical manifestations, etc.

The cases which I am about to describe were marked by unconsciousness, but the symptoms were of such a character as to exclude them from any one of the categories mentioned by Dr. Jackson. Yet no one, I think, after a consideration of their phenomena will hesitate to regard them as true epilepsy, however much opinions may differ from mine in regard to their localization.

Before proceeding however to discuss the important questions connected with the physiology and pathology of this form of epilepsy to which I ask attention, it will be well to describe briefly the cases upon which my views are founded.

CASE 1. M. I., a young woman about 22 years of age consulted me July 20, 1868, to be treated for what her mother informed me were "visions," which she was in the habit of having frequently every day. They occurred to her at the most unexpected times and were of great variety, no two, in fact, ever being exactly alike. While sitting in my waiting-room she had had, as

she said, one of these attacks, the principal feature of which was the hallucination of a large crane standing on one leg and attentively looking at her. After a few seconds a period of momentary unconsciousness occurred, on emerging from which there was entire recollection of the hallucination in all its particulars.

Inquiry showed that there were no vertigo or spasms of any kind, nor had there during the whole course of the seizure been any convulsive movements, or, in fact, any essential variation from the peculiar type I have mentioned. As soon as the hallucination appeared, and while still conscious, she conversed about its characteristics and was fully aware of its unreality. Suddenly, a loss of consciousness ensued, but there was no fixing of the eyes, no rigidity. Her head, if she were standing or sitting, fell forward on her chest, her breathing remained normal, there was no acceleration of pulse. In a few seconds she as suddenly raised her head, made a few coherent remarks and was entirely herself.

Usually a seizure lasted from twenty to thirty seconds, of which not more than five seconds were passed in unconsciousness. Sometimes they had been as long as a minute, but very rarely.

Such were the principal features as she described them to me. Further questioning of herself and mother led to the conclusions that there was no hereditary tendency, that the first attack had supervened four years previously, after a severe period of study at school, that her general health was good and that thus far there had been no mental deterioration. The attacks were, however, becoming more numerous and of somewhat greater duration. Menstruation had begun at 15 years of age and had always been regular.

I did not at the time regard these seizures as being epileptic in character, but before venturing to give a decided opinion as to their nature, I was anxious to see her while one was present. I accordingly made an appointment for her to come to my consulting-room the next day, after I had finished my morning's work, and to stay long enough for me to observe the active phenomena of an attack. As they came on with greater frequency in the afternoon I was pretty certain to be a witness of their characteristics during the time specified.

The next day, July 21st, in company with her mother she came to my residence. A few minutes after entering the consulting-room and while she was conversing in regard to some points in her clinical history, she exclaimed: "It has come, and oh! mother, what do you think it is? A beautiful chair covered

with red velvet all spotted with gold stars. It is just such a chair as I suppose is used for a throne. Well, this is the first time I ever had a chair appear to me. I have had rocks and all sorts of animals, but, but, but—" Here her head sank on her breast, her eyes were closed and her respiration became so quiet that it seemed for the moment to be suspended. There was no extraordinary paleness of the countenance and there had not been the slightest convulsive movement. In about three seconds she raised her head, smiled and looked as if nothing of an untoward character had occurred.

I at once formed the opinion that the paroxysm was a fraud, and I was indiscreet enough to say so in very plain terms. I told her that she had attempted to deceive me and I demanded her reasons for so extraordinary a performance. She and her mother both became indignant—justly so, I suppose—and left the house.

I saw nothing more of the case till November 18, 1878. She then, accompanied as before by her mother, again consulted me. She informed me that the symptoms had continued in an aggravated form ; that the " visions " lasted longer ; that the period of unconsciousness had been much more severe and prolonged, and that the paroxysms were of more frequent occurrence. Since her last visit to me she had, she said, consulted several practitioners, some of whom thought she was shamming, and others had regarded the attacks as hysterical.

As, during the interval which had elapsed between my interview with her, I had observed a similar case, in regard to which I had formed definite opinions, I determined to study the phenomena with care. I was satisfied, from the case referred to, that my first opinion relative to the present instance was erroneous, and that there was an actual morbid entity with very interesting and characteristic phenomena.

For over a month, therefore, I kept Miss I. under observation, scarcely a day passing that I did not see her. I had the opportunity of witnessing seventeen paroxysms. Sometimes they were preceded by a well-marked aura, and this was always a singular sensation, apparently somewhere within the cranium, but not capable of being exactly localized or described. This was never felt until within the last two years. It lasted only a second or two, and was immediately followed by the " vision."

The first paroxysm of this series which I witnessed was ushered in by the aura. She had hardly time to say " it's coming," when the hallucination began. She described it as consisting of a large

white bear in motion before her on the carpet. It seemed to be walking slowly to and fro, its head bent toward the floor as if scenting something. I closely watched her and could detect no spasm anywhere. She spoke clearly, without hesitation, and with entire distinctness. The pupils were normal.

I had taken out my watch to time the duration of the attack. Thirty-five seconds elapsed, and then her pupils suddenly dilated, her head fell forward, and her left hand, which she was at this instant pointing in the direction of the visional bear, dropped to her side. I pinched the skin of her face, then of each hand, without eliciting any evidence of cutaneous sensibility. I took up a fold of skin on each forearm just above the wrist and stuck a cataract needle which was at hand through it, with a like result. Her pulse—I had not felt it during the existence of the hallucination—was beating at the rate of about sixty per minute, and was full. Her face had not altered in color, nor was there any other change in it except such as was due to relaxation of the muscles —such as is present in sleep. The eyelids were closed, but not spasmodically. She remained in this state exactly twenty-eight seconds, breathing perhaps a little more slowly and deeply than before the accession of the paroxysm. Suddenly she raised her head, looked around inquiringly for a moment, and then, as if becoming aware of a sensation, looked at both her arms where I had pricked them. A drop of blood was oozing from each puncture. She asked what it was, and then, without waiting for an answer, exclaimed, "You have bled me." She was then entirely herself, and talked coherently and without the least excitement about the hallucination.

The only point in which there was any notable difference between this paroxysm and the one I had seen was the greatly increased duration of the period of unconsciousness, and I was informed by her mother that frequently this was prolonged to two minutes or more.

While making memoranda of the phenomena I had observed, and while she was walking up and down the floor, she said that she was going to have another attack, as she felt the peculiar sensation again in her head. She had no sooner uttered the words when the vision came. "It's a girl this time," she exclaimed, "a girl with long auburn hair and a cap on her head ; she looks like a French nurse. I think I will sit down, for if I do not I shall fall as soon as I become insensible," saying which she quietly sat down in a large arm-chair.

I pinched the skin of her right hand. "Oh!" she exclaimed, "I feel that, I am not insensible yet, I see everything in the room, as well as I do the girl who is not here, I can feel the least touch, and my hearing is as good as ever."

I asked her what "the girl" was doing, "O, nothing," she replied, "she is only standing there in front of the fire-place, looking at me."

I told her to shut her eyes, and then to tell me if she still saw "the girl."

"Yes," she answered, "just as distinctly as I did when they were open."

At forty-one seconds she became unconscious and remained in this state for one minute and five seconds, awaking—I say awaking, for her appearance was like that of a person asleep—suddenly and apparently in a normal condition of mental and physical health.

Several days elapsed before I saw her in another paroxysm. I then determined to ascertain the effects of certain measures directed toward arresting and preventing the attacks.

At the moment, therefore, that she notified me that a vision was coming, I caused her to inhale the vapor of the nitrite of amyl with the result of entirely preventing the further development of the seizure. I also discovered that if the amyl were inhaled during the presence of the halluciation the further progress to unconsciousness was immediately prevented, and the false appearance gradually faded away as the influence of the vapor became more powerful.

A like result was produced by inhalation of ether or chloroform and by firm pressure on the jugular veins. A band drawn tightly around the head was likewise a preventive.

At my suggestion she tried the effect of a strong volitional effort as an abortive measure and occasionally with success. It was necessary, she ascertained, that her mind should be entirely concentrated upon the subject, and her whole will power directed against the idea of the vision. If there was the least diversion of the attention, the paroxysm went on unchecked, and even under the most favorable circumstances she often failed to stop its course.

For the cure of the disease I prescribed the bromide of sodium, in doses of fifteen grains three times a day, with the effect of at once diminishing the number of the attacks and, at length, after about a month's treatment, causing them to cease altogether. She

now left the city, promising to continue the use of the bromide during a period of four or five months of absence. I neither saw nor heard more of her till January 7th of the present year, when she again came to consult me, accompanied by her husband, she having been married in the interval. She informed me that under the use of the bromide of sodium the visions had entirely disappeared; but that after continuing to take the medicine for over a year she had considered herself cured and had stopped it as much on account of the cutaneous eruption it had produced as anything else.

On December 2, 1879, she was married and on the 15th of the same month, while engaged in household work, had a return of the visions of much more severe character than she had ever had. There had been strong convulsive movements and loss of consciousness as accompaniments. Others similar had followed.

Her husband, who witnessed several of the attacks, stated to me that the unconsciousness, instead of as in the earlier stages of the disease, following the visions, was now contemporaneous with them, if not actually the first in order of sequence; that her face became very red and that she had no knowledge whatever of the nature of the seizure after the attack had passed of. The hallucinatory period lasted about half a minute, and was at once followed by the convulsive stage which persisted for a minute and sometimes more, the spasms being throughout of a clonic character and apparently equal on both sides. While the hallucination was present she acted and spoke as though it were a reality to her.

As yet, there were no indications that during these attacks she would attempt acts of violence against herself or others, but the last vision had been of a very terrifying character, and so far as I could judge, the paroxysms were now very similar to those of epileptic mania, and certain forms of what is called morbid impulse, with which neurologists are familiar. Epileptic paroxysms preceded by hallucinations, or of which such phenomena constitute a part, are not of very infrequent occurrence.

I at once recommended the use of the bromide of sodium in doses of fifteen grains three times a day, with, in addition, gradually increasing doses of the bromide of zinc, beginning with one grain with each dose of the bromide of sodium. The attacks at once ceased and up to this time the patient has remained in excellent health, without even the symptom of a paroxysm. She still takes the bromide of sodium as in the beginning, and the bromide of zinc in doses of eight grains three times a day.

CASE 2.—Mr. W., a prominent merchant of a neighboring city, consulted me March 1, 1879, for "nervous attacks," as he called them, with which he had been affected for several months. Upon inquiry I found that these "attacks" consisted of hallucinations of sight, smell and hearing, accompanied with numbness or tingling on the right side of the body, and followed immediately by periods of unconsciousness. There was no spasm of any kind and the speech was not in the least degree involved. His wife accompanied him and from her I heard many particulars of his clinical history.

Usually, but not always, the sense of hearing was the first to be deranged; at others that of smell took precedence. In whatever way the paroxysms begin the hallucinations of vision come last. For instance:

The patient would be engaged in his ordinary occupation, or, as was frequently the case, had just risen from bed on awaking in the morning, when, without other warning than a slight sound of tingling in the ears, he would have the hallucination of some one talking to him. For a moment, as was very natural, his intellect was imposed upon, and under the impression that his wife had spoken—the voice was very generally apparently hers or that of some other person who really was or might have been present—he would reply or ask what was said. On being undeceived he at once recognized the falsity of the sensation and knew what was coming. Directly after, he would experience the odor of fresh blood, and then, at once, came the hallucination of vision. About the latter there was no positive uniformity, though the appearances were always those of old people, either men or women, who seemed to come near to him, increasing in size as they approached, and then, as they reached him, he became unconscious. If standing, he fell, but the warnings were latterly invariably sufficiently prolonged for him to sit or lie down. When he recovered consciousness he had always a clear recollection of all the stages of the attack up to the loss of consciousness. His wife was positive that there were no convulsive movements anywhere, no rigidity, no fixing of the eyes and no confusion of ideas on returning to consciousness. The hallucinations, as well as I could gather, lasted about half a minute, and the periods of unconsciousness, something less than this. The last attack had occurred on the morning before the day of his visit to me.

He was an intelligent man, and I therefore asked him to describe minutely to me all the phenomena of this last seizure. I give his description as nearly as possible in his own words:

"I had risen from bed at about half past seven o'clock, and had just left the bath-tub, when I thought I heard my wife ask me if I had finished my bath; I was at the moment vigorously rubbing myself with a towel, and being in doubt about the exact words, I called out 'What did you say?' Immediately the words, 'Drown yourself, drown yourself; put your head under the water and hold it there.' I looked out of the bath-room door, but nobody was in the room. The last words, however, convinced me that an attack was coming on, for almost always I am commanded by the voices to inflict some injury on myself which is of a character to be suggested by my occupation at the time. Besides, at this instant I felt a kind of thrill pass through my right side.

"Knowing from experience what was at hand, I lay down on the floor, but not before the smell of fresh blood was perceived. It was a strong, overpowering, sickly smell, being accompanied with a slight sensation of nausea. It only lasted a few seconds, but before it was gone the vision came. I was lying flat on my back, looking up at the ceiling, when suddenly it appeared as though a large basket were descending toward me. It seemed to contain a little old black man, who leaned over the edge and grinned at me. When he got to within a foot of my face the basket began to ascend and another one, similar and with the same sort of old man in it, descended as did the first. It seemed then as though an endless chain were in motion, at regular intervals on which these baskets with little old black men in them were fixed.

"The movements seemed to continue for an hour or more, and then I lost consciousness. As a matter of fact, the whole seizure, from the beginning to the end, was only about a minute and a half. My wife came in just at the instant I became unconscious, and she is certain this condition did not last over half a minute.

"On regaining my senses I jumped up, took the towel and continued my rubbing as though nothing had happened. I felt as well as I ever had in my life; without, in fact, a single unpleasant feeling in any part of my body."

His wife stated that, hearing him call to her, she came from another room to him, and reached him just as he became unconcious. He lay on the floor perfectly still, without the slightest spasmodic movement anywhere, and when he regained consciousness was perfectly himself, mentally and physically.

I treated this gentleman with the bromide of sodium in doses of fifteen grains three times a day, with the effect of stopping the

seizures on the third day. Since that time till now (June 2d) he has had no paroxysm of any kind. He still continues to take the medicine.

Three other cases, similar in general features, but of which I have no full notes, have come under my observation.

That these are instances of epilepsy will not, I think, be questioned; that they possess peculiar features will be readily admitted. The nearest hitherto described form of epilepsy to this, consists of those paroxysms in which the patient has an hallucination usually of sight and then immediately passes into an ordinary seizure. Many such cases have been reported, and quite a large number have occurred in my experience. I will return to the consideration of these directly.

The main point which it is desired to bring forward in the present communication, relates to the seat of the intracranial disturbance. The title of "Thalamic Epilepsy" which I have given to this paper, sufficiently indicates my view of the matter. My reasons for the opinion held are briefly as follows—and are based as well upon physiology as upon such experiments as disease has made for us:

The thalami optici if centrally divided antero-posteriorly, will be seen to have imbedded in their substance four ganglionic masses. Of these, three are ranged along the superior surface of each thalamus and from their position may be designated the *anterior*, *middle* and *posterior*, while the other, more deeply placed, may be called the *central*.

Luys* who has studied the formation of the thalami optici with great thoroughness, designates these nuclei, from alleged anatomical and physiological relations, respectively, the *olfactive*, the *optic*, the *acoustic*, and the *sensitive* or ganglion of general sensibility.

It is true that Meynert† only half acknowledges their

* Recherches sur le système nerveux, etc., Paris, 1865, p. 198, *et seq.*

† Article on "The Brain of Mammals," in Striker's Manual of Histology, American Edition, p. 690.

existence, contending that the appearance of distinct nuclei is due to the mode of distribution of the fasciculi of fibres which enter and leave the thalamus, and that Huguenin * adopts this view of the subject. Really, however, the matter is of no great importance so far as its bearing on the subject matter of this paper is concerned. That the optic thalami, either by distinct nuclei or by themselves as bodies of ganglionic cells, are distinctly connected with the organs of the special senses referred to, as their nervous centres, is, I think, a matter capable of complete demonstration.

The relations of the optic thalami to sensibility were first pointed out by Magendie,† who ascertained that their irritation in animals produced excessive pain, while the other parts of the brain might be wounded without causing evidences of suffering.

They have also been regarded as specially the centres for vision, as presiding over the movements of the upper extremities, and again, as influencing voluntary movements in both the thoracic and pelvic limbs.

Although Todd, Carpenter and others have considered the optic thalami as centres for sensorial impression, Luys, ‡ more than any other physiologist, has elaborated this idea and has adduced arguments in its support which it is difficult to overlook. His doctrine is that the optic thalami are reservoirs for all sensorial impressions coming from the periphery of the nervous system, that like other ganglionic masses they elaborate these impressions and that by means of the fibres of the corona radiata, they transmit them to the cortex to be still further perfectionated by being converted into ideas. In his own language:

"All sensorial impressions after having been received and

* Anatomie des centres nerveux. French translation of Keller, Paris, 1879, p. 104.

† Leçons sur le système nerveux, Tome i, p. 103, *et seq.*

‡ Op. cit., p. 344, *et seq.*

concentrated in the gray substance of the optic thalami are irradiated toward the different regions of the cortical periphery. The white central fibres transmit them, and the gray substance of the convolutions receives and elaborates them." *

Many facts in morbid anatomy go to support this view of the relation between the several sensorial organs and the optic thalami. Twenty-six cases have been collected by Ritti,† from Hunter, Treviranus, Serres, Lancereaux, Cruveilhier, Andral, Marcé, Lallemand, Laborde, Luys, Voisin and others, to the effect of sensorial disturbances existing during life, in connection with disease involving the optic thalami, as discovered after death.

But it is not alone to morbid anatomy that we are to look for evidence of this relation. Experimental physiology equally tends to its establishment, and though the position of the optic thalami is such as to make it a matter of difficulty to act upon them as in case of the cortex, the obstacle has in a great measure been overcome by Fournié,‡ and we are thus placed in possession of data which have a distinct connection with the point at issue.

Fournié's method consisted in injecting by means of a hypodermic syringe, caustic solutions—such as a strong solution of the chloride of zinc—into the brain of a dog, observing the resulting phenomena, and then after death, carefully noting the part of the organ in which the injection had been deposited.

Seven of his experiments related to the optic thalami, and without referring to the other results, it may be stated that in every one there was a more or less complete loss of sensation.

Thus in case XV the left side was operated upon. The needle traversed the cornu ammonis, and the injection

* Op. cit. p. 346.

† Théorie physiologique de l'hallucination, Paris, 1874, p. 37.

‡ Sur le fonctionnement du cerveau, Paris. 1873.

was thrown out in the centre of the optic thalamus. As a consequence, there was complete abolition of all sensibility.

Such being apparently the physiological relations of the optic thalami, we come in the next place to discuss with something more of fullness the consequences, so far as sensation is concerned, of certain abnormal states of these organs. As I have said, Ritti has collected from various sources, many cases proving that injury or disease of the optic thalami leads to sensorial derangement, or the entire loss of one or more of the special senses. He has also gathered together from the works of Calmeil, Lagardelle and others, instances tending to establish the fact that hallucinations are the result of disease of one or both optic thalami. Several of the cases were supplied to him by M. Voisin, and had not previously been published. Of these latter I quote the following:

L. A., woman aged 41, entered the Salpétrière, January 30, 1867. Since 1865 she had been subject to hallucinations of sight accompanied at times by some excitement and partial alienation. At her admission, she had hallucinations of sight and hearing, and others connected with the genital organs. There were also delusions of persecution. Latterly the sense of hearing has been impaired. She died April 17, 1869, of typhoid fever.

Autopsy.—Neither thickening nor adhesions of the membranes; no sub-arachnoid effusion; cranial nerves normal, with the exception of the eighth pair, which were rotten at the most posterior and internal part of the two lobes of the cerebellum, and in the region nearest to the olivary bodies there were collections of little granulations such as are seen in the choroid plexus. These were continued as far as the floor of the fourth ventricle, where they covered its cerebellar wall. Nothing was found wrong with the left optic thalamus, but the gray anterior centre of the right thalamus was more than normally vascular, and in the part immediately subjacent to the olfactive centre of gray matter there was a spot, the color of the dregs of wine, due to a globiform extravasation of blood. In the middle region there was a lacuna. Each of these was over a millimetre and a half in diameter.

This case is instructive, not only on account of the situation of the lesion, but for the reason also that there was no other intra-cranial disease to which the symptoms could have been ascribed.

In a case that came under my own observation, a patient had hallucinations of sight and hearing, while at the same time he was both blind and deaf. This instance is, of course, not cited for the purpose of showing that hallucinations are not the result of disease or disturbance of the sensorial organ in anatomical connection with the sensorial aberration, a view held by the elder Darwin, Foville and others, but for calling attention to the important fact that *post-mortem* examination showed the existence of a clot, the size of a small bean, in each optic thalamus, while there was no other evidence of intra-cranial disease to which the symptoms could have been attributed, except atrophy of both optic and auditory nerves. In this case the blindness and deafness had been present for several years, but there were no hallucinations or other abnormal mental disturbance till, in the month of October, 1877, on successive days, the 12th and 13th, there were paralytic seizures without coma, on each occasion soon after waking in the morning. The motor paralysis was slight, and almost entirely disappeared in a few days, but the cutaneous anæsthesia was persistent to the day of his death—on December 10th following. I saw him first on October 15th, and again on the 18th in consultation with Dr. Nealis. The examination of the brain was made by me on December 11th, the day after death, which had occurred in the night. A second extravasation had taken place into the pons varolii, and had broken through the tissue into the subarachnoid space. This was evidently the immediate cause of death. The other lesions were as I have stated, and to them the blindness and hallucinations were evidently due.

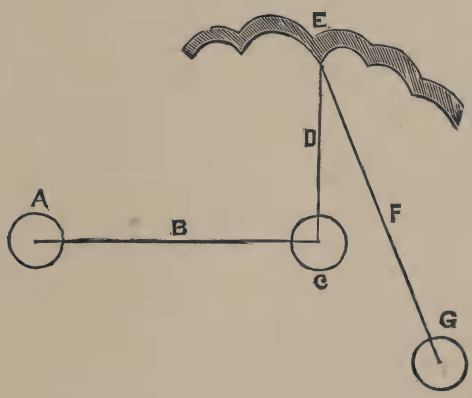
That the optic thalamus is the centre for perception as the cortex is for intellection is, to say the least, exceedingly probable. Every sense has these two stages in its full action. Something is *perceived*, that is one stage; it is more or less thoroughly *understood*, and that is the other stage. A pigeon, for instance, from which the cerebrum has been removed, leaving only the basal ganglia, perceives but

does not understand. A light may be held before the eyes, and the head is turned if the light be moved so that it can still be seen. If a loud noise be made near by, the animal starts or turns its head in the direction of the sound. These phenomena show *perception*, but they just as clearly show the absence of *intellection*, for the animal does not do the thing which, if it understood, it would do; it obtains no idea from the sensorial impression, and it is equally incapable of originating an idea, for it is devoid of consciousness. Consequently it gives no evidences of alarm, no matter how intense the visual or auditory excitation may be. *It perceives*, as is very evident from its actions, but these actions are such as to show that there is no further elaboration of the impression. It stops at the sensory ganglion because the intellectual ganglion has been removed.

The intrinsic starting point of every real sensorial impression is an organ of sense, such as the eye, the ear, or the terminal ramifications of the olfactory nerves. The starting point of an erroneous or false sensorial impression —illusion or hallucination—may be either the organ of sense concerned therein or the sensory ganglion—the optic thalamus. The cortex or intellectual centre for any sense cannot form a real or false sensorial impression. It can only elaborate the impressions which reach it from the sensory ganglion, and these are either true or false, real or unreal, according as they come originally from the ganglion, or are transmitted through it, from an organ of sense receiving real impressions from without, and according as the cortex is in a normal or an abnormal condition, will the ideas or beliefs which it forms from these transmitted impressions be normal or abnormal. It is true the cortex can recall former impressions and construct ideas from them, but here the idea is based on the recollection and not on a sensorial impression. Till, for instance, the eye and the optic thalamus had received the image of an American In-

dian and perceived it, the cortex could not have formed an idea of the appearance of such a being. All, therefore, that the cortex does, is to take cognizance of present or former sensorial impressions, which it receives or has received from the optic thalamus, and to form ideas from them. It does this normally when we bring the memory into action in a reasonable and logical manner; it does it abnormally, for instance, in delirium without hallucinations, but in which there is a constant recurrence, in a disorderly manner, of ideas previously formed from former sensorial impressions.

The accompanying diagram will tend to the elucidation of the views here expressed.



A is an organ of sense, the eye. Through the optic nerve *B* an impression received on the retina is transmitted to the sensory ganglion, the optic thalamus *C*, where it becomes a perception. From the optic thalamus it is transmitted by fibres of the corona radiata *D* to the cortex *E* where it is elaborated into an idea, and from the cortex another form of force may be evolved and an intelligent action may take place in consequence of the transmission through another set of fibres *F* of a motor impulse to a muscle

G. If there is no organ of sense there can be no normal sensorial impression; if the optic nerve be divided the sensation cannot be transmitted to the optic thalamus; if there be a diseased optic thalamus the sensorial impression will be perverted and there will be an illusion; if the cortex be in a normal condition this illusion will be corrected and understood as such erroneous perception; if, however, the cortex be diseased, the illusion will be accepted as true and a false idea or a delusion will be found. If the organ of sense receives no impression, but if such impression be formed in the optic thalamus, then we have an hallucination; if, again, the cortex be healthy, this hallucination is appreciated at its real value and there is a true idea in regard to it; if, on the other hand, the cortex be in an abnormal state, the hallucination is accepted for reality and a delusion is the result.

In the cases which form the basis of this paper there were hallucinations without intellectual derangement. They differed, therefore, from those cases of epilepsy in which there are hallucinations, and in which these hallucinations are received as realities, and acts, perhaps of violence, committed in logical sequence with the delusions formed. These doubtless have their starting point in the optic thalamus, as shown by the existence of hallucinations, but the morbid process soon passes to the cortex, and the resultant phenomena are loss of consciousness and intense intellectual and motor disturbance.

I feel warranted, therefore, in concluding that there was in each of my cases no lesion of any of the intellectual sensorial centres situated in the cortex, but that the disease was confined entirely, or nearly so, to the optic thalami. I say nearly so, because the loss of consciousness which ensued showed that there was that necessary cortical disturbance without which there can be, in my opinion, no true epilepsy. Hallucinations without loss of consciousness no

more constitute epilepsy than twitching of the hand or a stench in the nostrils, similarly unaccompanied, is epilepsy. Either may become epilepsy by further extension of the morbid intra-cranial action, but either may exist indefinitely without such extension occurring.

Again, an additional argument against the involvement of the cortex is found in the fact that there were no muscular spasms in either case. Muscular spasms are, of course, not epilepsy, but muscular spasms accompanied by unconsciousness make a true epileptic paroxysm. In these cases the motor disturbance was substituted by sensorial disturbance, and consequently there was sensorial epilepsy, or, as I prefer to call it, from the probable seat of the lesion, thalamic epilepsy.

I have not in this paper discussed the question of the situation in the cortex of the various intellectual sensorial centres. Properly, it has no connection with the subject, and again, there is great doubt as to whether a single one of them can in the present state of our knowledge be definitely located.

And it is scarcely necessary for me to add, that the views here expressed relative to the location of the lesion in cases of sensorial epilepsy, are entirely opposed to those of Hughlings Jackson and Ferrier, who regard such cases as of cortical origin. These eminent authorities disregard altogether the researches of the French physiologists and pathologists, and do not make the distinction between perceptional sensation and intellectual sensation which I have contended for in this memoir, and which I think is logically and psychologically proven to exist. Thus, Ferrier in citing a case from Bell, says: *

"I mention this case chiefly because it harmonizes with the observations of Hughlings Jackson, already referred to,

* The Localization of Cerebral Disease, p. 131.

in respect to the frequent association of optical illusions, colored vision, etc., with disease of the posterior lobes. These spectra are the counterpart of the motor discharges caused by irritative lesions of the motor centres. That they should occur more particularly with lesions situated toward the posterior aspect of the hemispheres is quite in accordance with the localization of the visual centre in the angular gyrus. These sensory discharges in connection with epilepsy of cortical origin, whether in the domain of sight, hearing, smell, taste or tactile sensation, are without doubt to be looked upon as indications of irritative lesion of the sensory centres, though we have not yet sufficient material to enable us from a purely clinical point of view to connect any peculiar form of sensory discharge with a specially localization lesion, unless we regard it as established in respect to optical illusions."

Now, Dr. Ferrier* has himself cited a case which is entirely in opposition to his views of cortical localization in sensorial epilepsy, and just as strongly in favor of those set forth in the present paper. It is so apposite that I trust to be excused in giving a tolerably full abstract of the details:

The patient, a soldier, was admitted for epileptic insanity. "States that he saw dogs and cats about him; continually trying to tear the bed clothes or to seize hold of his own throat; every five or ten minutes he has epileptiform seizures, during which he grows very violent, requiring restraint.

"Tries to seize the poker or anything else that he may strike those in attendance on him. Endeavored to jump out of the chamber window. He has previously been in the asylum.

"As far as can be ascertained he labors under no delusion, but is decidedly demented. For certain events, such as his former residence here, his memory appears good enough, but for more recent events he possesses not the slightest knowledge.

"But the most peculiar feature in the case is the partial epi-

*Pathological Illustrations of Brain Functions, in West Riding Lunatic Asylum Medical Reports, vol. iv, p. 31.

leptic seizures which occur every five or ten minutes without any loss of consciousness. The patient is suddenly seized with a convulsion in the left arm, the head is turned to the left as well as the eyes, and occasionally the muscular movements spread to the legs and right arm, but in a very slight degree. Indeed, after each seizure the patient's respiration is heavy, but there is not the least degree of unconsciousness, though he appears inclined to drop off to sleep. The patient is unable to stand, muscular power being entirely lost in left leg and decidedly diminished in left arm."

The patient went on from bad to worse; had long periods of drowsiness and stupor; during one night had ninety fits; spoke of visions of dogs, etc., being in his room all day, and on January 17th died.

Now here was a case of epileptiform convulsions attended with paralysis and hallucinations. It was one in which, according to Drs. Jackson's and Ferrier's views, there ought to have been well defined cortical lesions; it was one in which, according to the views expressed in this paper, there ought to have been lesions of one or both optic thalami. Taking into consideration the facts that the hallucinations in this case were conjoined with left motor spasms and paralysis, I would not have hesitated to diagnosticate the existence of lesion of the right optic thalamus. Now, as a matter of fact, let us see what was revealed by the *post-mortem* examination, which, as Dr. Ferrier tells us, was made with the utmost care and thoroughness.

"*Head.*—The skull is of average thickness and density and is fairly symmetrical. The dura mater is somewhat adherent and the sinuses contain only fluid blood. There is little or no thickening of the arachnoid, but there is a good deal of superficial wasting of the convolutions of the frontal and parietal lobes. The vessels at the base are perfectly normal. There is no visible hyperæmia and pia mater strips with great ease. The gray matter is somewhat pale; it as well as the white matter is of fair consistence. There is no trace of softening, clot, induration or other organic change anywhere visible, although the most careful search is made. The ventricles are of average size and contain only a

small quantity of fluid. *The optic thalamus on the right side is smaller than its fellow on the left and its posterior end is attenuated.** The whole brain weighs 47 oz.; right hemisphere, 20¾ oz.; left hemisphere, 20½ oz.; cerebellum, 5¾ oz.; pons varolii, 4 drachms, 24 grains; medulla oblongata, 2 drachms. The medullary substance of the brain is perhaps a little firmer than it ought to be. No pathological change can be detected in the cerebellum, pons or medulla."

Certainly no more striking case in support of the doctrines set forth in this paper could well have been supplied even if it had been made to order.

It is true that Ferrier † regards the optic thalami both from his own experiments and those of others, as being intimately connected with all sensorial functions, but they are so in his opinion merely because they are "ganglia of interruption," or centres of convergence for the sensory tracts on their way to the cortex, and not the nerve-centres or generators of nerve force. If this view were correct, it is difficult to see what special use they can have, for the sensory tracts would certainly get to the periphery just as well without as with them, and what is more, nature would have to bear the odium of having allowed the construction of two entirely superfluous masses of ganglionic tissue.

I have as yet said nothing of Nothnagel's experiments relative to the functions of the optic thalamus. The results obtained by this brilliant experimental physiologist are so exceptional in character as to be quite inconsistent with those of all other observers, for they indicate no especial function as belonging to the organ in question. This fact is perhaps explained by a consideration of the method employed.

In his recent work Nothnagel ‡ considers the question at considerable length. Among his conclusions, the following

* Italics are mine.

† The Functions of the Brain, New York, 1880, p. 259, *et seq.*

‡ Topische Diagnostik der Gehirnkrankheiten, Berlin, 1879, p. 222.

sufficiently express his views so far as the scope of the present inquiry extends:

1. "In regard to the majority of the symptoms which have been ascribed to lesions of the optic thalamus, it is very doubtful whether they directly or only indirectly through involvement of neighboring parts have such origin; and other symptoms properly associated with thalamic lesions are of doubtful import as they also occur from injury or disease of other organs.

2. "From which it follows that a certain diagnosis of lesions restricted to the thalamus is at present generally impossible, for only under a specially favorable combination of circumstances is it possible to make such diagnosis with any degree of surety."

And if I were specially discussing the functions of the optic thalami in all their relations, I should not overlook the valuable contributions of Dr. Crichton-Brown* to our knowledge of the subject.

In conclusion, I think the following deductions may fairly be drawn, at least for the present:

1. That there is a form of epilepsy, the phenomena of which are simply hallucinations and loss of consciousness.

2. That the morbid anatomical basis of this type is seated in the optic thalamus.

* "The Functions of the Thalami Optici," West Riding Lunatic Asylum Medical Reports, vol. v, 1875, p. 227.

INDICAN IN HEALTH AND DISEASE.

By H. N. HEINEMAN, M.D.,

ASSISTANT TO LABORATORY OF THE ALUMNI ASSOCIATION OF THE COLLEGE OF PHYSICIANS AND SURGEONS, N. Y.

INDICAN is a normal organic constituent of the urine. Schunck in 1857 first called attention to it and proved it to be a compound body.

Composition.—It is composed of indigo-blue, indigo-red, indigglucin, leucine and acids. The first two are coloring matters, of which the former is identical with Heller's uroglaucin, and the latter with Berzelius' urrhodin.

Indican has been separated from the urine in the form of small branching white shining masses by Bauman and Brieger,* who have determined its chemical formula to be $C_8H_6NSO_4K$. They consider it to be the combination of the ether sulphate of indol-hydroxyl with an alkali, and have proposed the name of indoxyl sulphate. It ranks among the aromatic series of compounds to which phenol and benzol belong.

Source of Indican.—Jaffe, Kuhne, Salkowski and others have demonstrated that indican is derived from a nitrogenous substance called indol. Indol appears in the intestines and is also found in the fæces, the peculiar odor of the latter being due to its presence. Kuhne proved that it is a product of the decomposition of albuminous substances acted upon in the intestine by the pancreatic juices.

* *Zeitschrift f. Phys. Chemie*, iii, p. 254.

Brieger * succeeded in obtaining it, without the body, from the simple decomposition of the liver of the horse, maintained at a temperature of 40°, and with a feeble alkaline reaction. It is absorbed from the small intestine and eliminated by the kidneys under the form of the conjugated compound indican. Indol as well as its derivative indican, both belong as we have already stated, to the class of aromatic substances.

Among the experiments (of which those of Christiani † are the most recent) to prove the relation of indol and indican, has been the introduction of the former substance, by hypodermic injection and otherwise, into the lower animals, when invariably an increase in the excretion of indican followed. Other substances chemically allied to indol, have also been similarly used and have produced some increase in indican, but this was never so pronounced as when indol itself was employed.

Among the latter substances may be mentioned phenol, benzol (Christiani and others), creosote and bitter almonds (Kletzinsky). The first of these is also normally found in the intestine, produced in the same manner as indol, and it reappears in the urine to some extent, whether artificially injected or absorbed from the intestine, under its own form.

Recently Ewald ‡ in a case of artificial anus, after withdrawing the secretions from the upper portion of the small intestine found that neither indican nor phenol appeared in the urine. When the secretions were permitted to pass through the intestine, both substances were present. This experiment he claims further demonstrates both the derivation of indican from indol, and also that indol is produced in the lower portion of the small intestine only.

In opposition to what would naturally be supposed, in

* *Zeitschrift f. Phys. Chemie*, iii, p. 134.

† *Ibid*, ii, p. 272.

‡ *Virchow's Archiv*, lxxvii, p. 409.

the herbivora in which indican is largely (Jaffe) present in the urine, Brieger * found phenol and indol always in small quantities and present in the rectum and fæces only, and not in the contents of the intestine.

Christiani and other investigators of whom mention has already been made, have demonstrated that even after a prolonged exclusive meat diet, indican was excreted and in increased quantity.

Other sources than indol, and organs other than the kidneys for the production of indican suggest themselves. The blood, from which all other coloring matters of the body are derived, may yet prove to be an additional source of the coloring matter, indican. A coincidence which is so striking that it cannot be considered as accidental, would tend to confirm this possibility. Thus many diseases,† which are attended by a diminution of the quantity of hæmoglobin of the blood, furnish a corresponding increase in the amount of indican. And in some diseases, such as pleurisy and peritonitis, the diminution of the first at a particular stage, coincides exactly with the period of time when indican is generally found in greatest abundance. Hippuric acid, a substance which Jaffe has suggested may be derived from the same material as indican, with which it is frequently (Jaffe, Senator) found associated, is formed in other tissues besides the kidneys, according to the recent experiments of Salomon.‡ Further experiment may likewise demonstrate other tissues of the body as the seat of formation of indican.

Color.—Indican does not impart any color to the urine, by which its presence can always be predetermined. In rare instances, owing to its decomposition by a ferment either before or after being voided from the bladder, it

* l. c.

† See Quinquand's article, ARCHIVES OF MEDICINE, vol. iii, No. 1, p. 32, 1880.

‡ *Zeitschrift f. Phys. Chemie*, iii, p. 366.

gives to the urine a blue tint. In such cases, which I have met with, crystals of indigo-blue are precipitated. These are very small blue needle-shaped crystals, usually aggregated and forming radiated masses, and have no action upon polarized light.

We must carefully distinguish by microscopic examination the blue color sometimes given to urine by the presence of a fungus.

It is stated by Neubauer and Vogel* that urine containing indican is very generally yellow in color. In 255 examinations I have noted the color when indican was present. Of these

101 specimens had a decidedly yellow tint,

64 were dark amber,

57 pale or straw colored, and

33 pale amber.

The aggregate of the other colors preponderated, though, as will be seen, there was a predominance of the yellow over any one other color.

Tests.—Owing to the readiness with which indican is decomposed by the mineral acids, its recognition is made quite easy.

The determination of the amount of indican is deduced from the quantity of indigo-blue which is set free. It has been well established by Hoppe-Seyler, by means of spectroscopic examination, that the indigo-blue always bears a definite ratio to the quantity of indican. The amount of indigo-blue is judged simply by the intensity of the blue color imparted to the menstruum by which it is dissolved. Generally it is the depth of color of the chloroform, this being the general solvent employed.

Jaffe, Salkowski and Senator have each in succession made some improvements in the tests employed. Hennige has added a few suggestions, and I have some to make.

* Analysis of the Urine, last edition, p. 67, N. Y., 1879.

Senator's Test.—Pour 10 cc. of urine into a deposit glass and add slowly, while briskly stirring the mixture, 10 cc. of pure hydrochloric acid. In the course of a few minutes, sometimes longer, the mixture acquires a violet or bluish tint, according to the amount of indican present, and fine flocculi of indigo-blue may be seen to be precipitated. Next add drop by drop a few drops of a saturated solution of calcium hypochlorite, which causes still further decomposition of the indican and increases the depth of color of the mixture. The calcium solution is added until we have obtained what we suppose the bluest tint, generally alone determined by the fact that the mixture begins to lose color when we have added a trifle too much. Next we add a small quantity of chloroform, agitate it with the mixture, this will dissolve all the indigo-blue which has been set free, carry it down with it to the bottom of the glass, and leave the indigo-red in the mixture above. We now pour off the supernatant liquid, leaving the chloroform with its dingy blue color behind. By passing the chloroform, according to the suggestion of Hennige, through torn bits of fine filtering paper, we have left a clear blue solution.

Weber's Test.—"W. Weber* employs a method of detecting indican, which is especially valuable in those cases in which the quantity present is very small. To 30 cc. of urine in a large test-tube add an equal volume of concent. hydrochloric acid, and warm the mixture. One or two drops of nitric acid increase the sensitiveness of the test. The mixture is then cooled by holding it in running water. A layer of ether, 2 or 3 cc., is poured upon it, and the mixture shaken. Add a few drops of alcohol if the blue color is not evident in a few minutes. After a while the indigo-blue separates from the ether and forms a deposit between the two fluids, while the indigo-red remains dissolved in the ether."

* *Archiv der Pharmacie*, Oct., 1878, p. 340. *Bost. Med. and Surg. Journal*, 1879.

Modification of Senator's test.—I have obtained more accurate results and avoided the necessity of repeating examinations, by the following suggestions:

First. Add the chloroform after the acid has been added.

Subsequently add the solution of calcium hypochlorite, and after each drop agitate and allow the chloroform to settle so as to recognize its color.

In this way, even if the mixture should begin to fade from having added a drop too much of the calcium solution, we have only to remember the depth of color which the chloroform had acquired just previous.

A ready test for the ordinary practitioner, and which, while it will not indicate the presence of small quantities, will show any increase, is found in the following. The faintest blue tint here represents a slight increase.

Fill a good-sized ordinary test-tube one-third full of urine, add equal amount of hydrochloric acid (c. p.) slowly and with constant shaking, warm slightly and then cool, when the specimen assumes the violet or bluish tint. Next add from 30–60 drops (according to size of tube) of chloroform, and after shaking the mixture thoroughly, allow it to settle and observe the color of the chloroform. Now add drop by drop a dilute solution of calc. hypochlor., agitating and continuing until the chloroform has acquired its deepest blue tint.

Remarks upon the tests.—1. Dark and bile-stained urine should be first decolorized by plumbic acetate, not used in excess. Albumen must also be separated.

2. Decomposed urine must not be employed.

3. The resulting color of the chloroform sometimes varies from that described, a greenish black and red (the color of indigo-purpurin) being obtained. In all of these cases filtration through bibulous paper will invariably leave some tint of blue upon the filter, showing the presence of indigo-blue.

4. We have no means better than the judgment of the eye for determining the depth of color of the indigo-blue. Thus a deep blue indicates a marked increase, a very faint blue slight or normal, a decided blue a moderate increase. Scales of color, for comparison, have proven useless in the hands of Hennige and others.

5. The suggestion of Hoppe-Seyler that the degree of concentration of urine determines in a measure the reaction found, must be borne in mind.

Through the kindness of the resident staff of the Roosevelt, New York and Bellevue Hospitals, I have been enabled to examine upward of 200 cases, of which more than 150 were in cases of disease.

I have employed Senator's test with Hennige's suggestion of filtering the chloroform deposit. Weber's test has occasionally been made use of, simply as a means of comparison and to appreciate its delicacy. But the use of the large quantities of acid, which this latter requires, and the resulting atmosphere of chlorine in which the experimenter is placed, made it too obnoxious for constant use.

Each case has been examined at intervals, three or more times. Many specimens were repeatedly reëxamined in order to insure accuracy.

In reviewing the literature I find a serious cause for regret in the indefinite results and conclusions given by many investigators. Instead of stating with positiveness the exact number of cases seen and investigated, the terms, frequently, in several cases, in numerous instances, etc., are used. Still further the proportionate increase of indican, whether moderate or marked, is not always stated.

With the view of assisting future investigations I have arranged the appended table, which includes 396 cases investigated by Hoppe-Seyler (100 cases of normal urine), Senator, Hennige and myself. The results of the numer-

ous investigations and invaluable labors of Kuhne, Salkowski, Heller, Scherer, Virchow, Edlefsen, De Vries and others, I shall compare with the deductions to be made from the above table. In some instances, conclusions already arrived at have been corroborated, in others additional investigations have modified previous inferences.

First, in 149 cases of normal urine, of which Hoppe-Seyler investigated 100, indican was present in all but 14. Of 49 cases reported by me, in 21 there was an increase above the average normal amount, and in 9 a very marked increase.

Next, in diseased states :

Circulatory system.—Of 5 cases of cardiac disease 3 exhibited a normal amount, 2 a moderate and marked increase respectively. In one case of aneurism, indican was absent.

Respiratory system.—Of 6 cases of pleurisy, pneumonia and empyema, 2 had a normal quantity, 2 a moderate and 2 a marked increase. Senator states that in pneumonia and pleurisy he has found a moderate increase.

Of 13 cases of acute and chronic phthisis 2 showed marked, 3 moderate increase, 5 a normal or slight amount and 3 absence. Hennige states that the condition of the intestine, whether diarrhœa be present or not, determines the increase in phthisis. Senator, with whom I agree, states with or without diarrhœa there is a moderate or marked increase.

Digestive system.—In 3 cases of stomach dyspepsia and 1 of dilatation there was marked increase. In a few cases of ulcer of the stomach, Senator has met with moderate increase. In numerous cases (number not stated) of gastro-duodenitis, Hennige found very marked increase. In 1 case I have found absence of indican and in 3 marked increase. In 5 cases of diarrhœa, there was marked increase in all. Of 3 cases of dysentery, 2 exhibited a

slight amount and 1 none; of 4 cases of chronic constipation, 2 exhibited absence, 2 marked increase. Senator, and with him, Hennige, Jaffe, Edlefsen and DeVries, find that in simple constipation from atony, without invagination, only a slight amount of indican is present. The result in 2 cases met with by me is at variance with this. In 1 case of intussusception the increase was very marked. In 7 cases of peritonitis there was marked increase, and this was greatest in the acute cases and in those which were general rather than those that were localized. In 4 cases of intestinal hemorrhage there was marked increase. In 1 case of cholera morbus there was marked increase. Senator, Wyss and Gubler have found marked increase in this and in cases of Asiatic cholera.

Liver.—Of 9 cases of cirrhosis, 2 only exhibited a moderate and marked increase respectively, 2 were normal, and in 5 there was none.

Genito-urinary system: *Kidneys.*—Of 16 cases of Bright's disease, 10 presented a marked and 4 a moderate increase, in 2 indican was absent.

Senator states that he has found no increase in any except cases of atrophied granular kidney. Heller, Scherer and Virchow had previously reported increase in indican in kidney lesions other than the one just mentioned. My own cases confirm this latter view. Of 7 cases of Addison's disease, 3 had moderate and 4 marked increase.

Bladder and Urethra.—Of 12 cases in which pus was found in the urine in connection with urethral stricture, cystitis and gonorrhœa, 7 gave moderate and 1 marked increase, 3 normal or slight amount, and 1 none.

Uterus and Ovaries.—1 case of uterine disease had marked increase; 1 case of puerperal fever, normal or slight amount; in 2 cases of ovarian tumor none, and in 3 cases of pregnancy 1 had marked and 2 moderate increase

Osseous system.—Of nine cases of arthritis, caries and necrosis, 4 had moderate increase, 3 normal or slight amount, and 4 none. Of 6 cases of suppuration and cellulitis unconnected with bone disease, 3 had moderate increase, 2 normal amount and 1 none.

Nervous system.—Indican was absent in 1 case of apoplexia meningea; markedly increased in 1 case of prog. muscular atrophy, in 1 case of insanity and epilepsy. In 3 cases of paraplegia, 2 had marked increase and 1 normal amount. In 1 case of sciatica, the quantity was normal. In 2 cases of cerebral tumor and 1 of hypochondriasis and nocturnal emissions there was absence. Several authorities claim that spinal irritation is generally accompanied by an increase of indican.

General diseases.—Of 5 cases of alcoholism, 2 had moderate and 1 marked increase and 2 were normal.

Of 20 cases of rheumatism, 8 of 10 cases of acute disease were normal, 1 had marked increase and 1 none. In 4 of 10 chronic cases the quantity was normal or slight, markedly increased in 4 and moderately in 2.

Of 19 cases of malaria, 6 were normal, 5 had moderate and 1 marked increase and 7 had none. Of 6 cases of typhoid fever, 1 had moderate and 5 marked increase. Hennige has found the increase in typhoid fever dependent upon the presence of diarrhœa. Senator and I have found it independent.

In 1 case of convalescence from yellow fever the quantity was normal. Of 10 cases of constitutional syphilis, 4 had moderate and 1 marked increase, 2 were normal, 3 had none.

Of 6 cases of chlorosis, 3 had moderate increase and 3 normal or slight amount. There was a normal or slight quantity in 2 cases of Werlhof's disease, marked increase in 1 case of progressive pernicious anæmia and absence in 1 case of leucocythæmia. In 3 cases of trichinosis and 2

of lead poisoning there was marked increase. In 1 of chronic arsenical poisoning, absence.

In 21 cases of carcinoma interna, there was very marked increase in 19 and absence in 2.

In 3 cases of abdominal lympho-sarcoma and in 1 case of osteo-sarcoma there was marked increase in all.

Conclusions.—Indican is only exceptionally absent in health. It may vary in quantity in the healthy individual, generally being small in amount, but occasionally as marked as in disease.

Certain diseased conditions tend to produce a decided increase.

The most marked increase is obtained in those diseases which affect the alimentary canal, and more especially the small intestine, whether they be local or general diseases with local lesion. Among these are dyspepsia, gastro-duodenitis, chronic constipation, intussusception, diarrhœa, peritonitis, cholera, lead poisoning and typhoid fever.

In diseases causing inanition, as phthisis and other prolonged suppurative diseases, as caries and necrosis, marked increase is found.

Among the diseases producing altered blood states, some, such as malaria, syphilis, rheumatism and alcoholism, progressive anæmia and chlorosis, although *always* causing the most profound blood-changes, *do not always* effect a decided increase. This is a singular exception, to which Senator has called attention, but the investigations are not yet sufficiently numerous to draw conclusions.

Certain nervous disorders, as insanity, epilepsy, Addison's disease, progressive muscular atrophy and paraplegia, cause marked increase.

Disordered function of the kidneys, as in chronic Bright's disease, causes marked increase.

Internal malignant tumors, as carcinomata and sarcomata, cause increase most markedly and constantly of all.

The explanation of the cause of the increase in various diseases is not as yet adequate to satisfy all cases.

The experiment of Jaffe of ligating the small intestine, by which the amount of indican excreted was increased, demonstrates that conditions retarding peristalsis and favoring the absorption of indol, cause an increase of indican. This reasonably explains the result in cases of peritonitis, constipation and similar disorders.

How, satisfactorily, to account for the result in other diseases must, I regret to state, still remain unanswered.

The theory of Hennige that other diseases act through the nervous system and produce a change in the nature of the pancreatic secretions, and thus cause an increase, is effectually answered by Senator. He calls attention to the fact that indol, the mother substance of indican, is a product of decomposition. Furthermore, Brieger has shown that indol is produced without the presence of pancreatic juices.

Possibly in changes in the circulating fluid, rather than in local disturbances of the system, we may find the reason for the increase in the remaining diseases.

	Hennige	Heineman	Hennige	Heineman	Hennige	Rosenstirn	Heineman	Hennige	Carter	Heineman	Salkowski	Rosenstirn	Hoppe-Seyler	Senator	Observers who have not reported exact statistics
	None		Normal or Slight		Moderate Increase			Marked Increase							
															Schunck, Hoppe-Seyler, (100 *cases*)
Normal		11		17			12			9					Senator, Carter
Cardiac Dis.				3			1			1					
Aneurism (Aorta)				1											
Ac. Pneumonia				1											
Pleurisy and Empyema				1			1			2					
Ac. Miliary Tuberculosis.			2								1				
Phthisis		3		3			3			1					Senator
Dyspepsia										3					
Dilatation of Stomach										1					
Gastro-duodenitis		1								3					
Diarrhœa								4		1					Senator
Dysentery		1		2											
Constipation chr.	2									2					
Intussusception									1						
Cholera Morbus								1							Gubler, Wyss, Senator
Peritonitis								5			2				Kletzinsky
Cirrhosis	5			2			1			1					
Jaundice			4												
Chr. Bright's Disease		2					4			6				4	Rosenstirn, Gubler, Kletzinsky
Stricture Urethræ				1			4			1					
Cystitis Vesicæ				2			3								
Dilatation Vesicæ		1													
Gonorrhœa							1								
Malaria	7			6			5			1					
Typhoid Fever							1			5					
Yellow Fever				1											
Syphilis		3		1			4			1					
Leucocythæmia		1													
Pern. Anæmia Prog.								1							Rosenstirn
Werlhof's Disease			2												
Chlorosis			3		3										Rosenstirn
Trichinosis								3							
Lead Poisoning								1		1					
Arsenical "			1												
Sarcoma of Thigh										1					
Carcinoma Interna		1						2	1	3			1	12	Neftel
Lympho-Sarcoma of Abdomen											1			1	
Pregnancy							2			1					
Uterine Disease										1					
Ovarian Tumor	2														
Puerperal Fever				1											
Arthritis					1										
Caries & Necrosis		1		3	1		1								
Goitre				1											
Apoplexia-Mening.	1														
Prog. Musc. Atphy								1							
Addison's Disease						3		1				3			
Insanity										1					
Epilepsy										1					
Paraplegia		1		1						2					
Sciatica				1											
Cerebral Tumor	1	1													
Hypochondriasis		1													
Hæmoptysis	3														
Intestinal Hemorrhage								4							
Suppuration		1		1	1										
Cellulitis				1	1		1								
Alcoholism				2			2			1					
Ac. Art. Rheu'sm.		1		8						1					
Chronic Art. "															
Subac. " "				4			2			4					

BIBLIOGRAPHY.

Edw. Schunck. On the occurrence of indigo blue in urine. *Phil. Mag. and Journal of Science*, IV, sev. vol. xiv, 1858. *Heller's Arch.*, 1846. *Wiener Med. Wochenschrift*, 1859. No. 27.

N. Kletzinsky. Ueber die Chem. Constitution und semiot., Bedeutung des Harnindigs.

Th. A. Carter. Notes of two cases. *Edinb. Med. Journal*, July, 1858.

Carter. On indican in the blood and urine. *Edinb. Med. Journal*, August, 1859.

Hoppe-Seyler. Ueber Indican als constanten Harnbestandtheil. *Virchow's Arch.*, 1863. Heft 3 u 4, p. 388.

Gilchrist. On indigo in the urine. *Edinb. Med. Journal*, December, 1861. *Du Bois-Reichert's Archiv*, 1870. Heft 1. *Pflüger's Arch.*, 1870.

Jaffe. Ueber den Nachweis u. die quant. Bestimmung des Indicans. Bericht der deutschen Gesellschaft. Bd. 8. *Centrabl. f. die Med. Wissenschaft*, 1872, Nos. 1 and 31; 1875, No. 36; Salkowski, 1876, No. 46; Senator, 1877, Nos. 20, 21 and 22; 1878, No. 25. *Arch. der Heilkunde.* Bd. 9, p. 232. *Virchow's Arch.*, 1872, p. 27; 1876, Heft 3; 1877, Heft 1. *Deutsch. Archiv. f. klin. Med.*, xv, p. 403.

Wolffberg. Ueber die Verminderung der Indigoauscheidung durch den Harn bei innerlichem Gebrauch der Salicylsäure.

Teurosch. Beiträge zur Lehre über die Entstehung des Indicans im Thierkörper. Königsberg, 1877.

Lœbisch. Leitfaden der Harnalyse. Hoppe-Seyler, Handbuch der chem. Analyse.

Neubauer. Vogel. Harnalyse. *Berlin. klin Wochenschrift*, 1878. No. 25. Taken from Hennige, Die Indicanauscheidung in Krankheiten. *D. Arch. f. klin. Med.*, 1879, p. 287.

Weber. *Arch. der Pharmacie*, October, 1878, p. 340; *Bost. Med. and Surg. Journal*, Feb. 27, 1879; *Centralblatt f. die Med. Wissenschaft*, 1880. No. 1, pp. 6 and 7. Nos. 12 and 13.

WATER AS A PROPHYLACTIC AND A REMEDY.

By S. G. WEBBER, M. D.

THE subject of water drinking seems worthy of more than a passing notice. Many people have a notion that it is injurious to drink at meals. Of course one may swill down such an immense quantity of water that it shall be injurious, but a moderate quantity of fluid taken at meals is rather beneficial; the abstinence advocated by many is injurious.

A large class of patients are affected with symptoms of an undefined character, a vague unrest and disquiet showing itself by discomfort or even pain, sometimes in one place, sometimes in another; they are usually also subject to constipation; often have an unhealthy hue of the skin; they are frequently classed as hypochondriacal or hysterical; there is no well defined disease. It has long been my custom to inquire of patients thus affected as to the amount of drink they take and how much urine they pass. Often the amount of drink is much below the average; there is a tendency to dryness of the skin; the urine is scanty, high colored and strongly acid, sometimes depositing a sediment on standing. Under the use of an increased amount of water the perspiration is increased, the urine becomes more natural and the unpleasant symptoms diminish or disappear.

The waste of tissue-changes in the system must pass into the blood and can leave the system only in solution. Dur-

ing comparatively good health the amount of blood is maintained at nearly the same figure, only so much water will be parted with through the skin, lungs and kidneys as can be restored from other sources. If too little water is ingested the perspiration will be slight, the elimination of urine will be diminished, and the excretion of waste material will be lessened. The blood will be continuously saturated with the results of disassimilation or nearly so, the removal of the waste of tissue-changes is not accomplished with sufficient regularity, and the tissues become clogged with used-up material, and nutrition is interfered with. The balance each day against health is very slight, but finally there is such an accumulation that unpleasant symptoms are developed. If the person continues to eat heartily, either the surplus food passes off by the intestines, or is deposited in the shape of fat, the nitrogenized portions assisting to load the urine with urea and urates. Let a person drink a larger amount, and, the blood having a sufficient supply of water, more urine is excreted ; the loss is made good to the blood by absorption, and a larger amount of waste products is taken up to be eliminated ; more urea, phosphoric and sulphuric acids pass off by the urine, which is increased in amount ; there is more disintegration of the tissues. This loss is made up by new material, so nutrition is increased.

Water taken with the food favors digestion ; when taken into the stomach a part is absorbed by the gastric vessels, carrying with it the soluble constituents of the food. So much as is not immediately absorbed assists in softening and breaking up the larger particles of food, and thus aids in the gastric digestion by facilitating the action of the gastric fluids. A portion of the water is carried into the intestines with the semi-digested food and acts favorably in the same way ; also, the blood being well supplied with water, the fæces are not so hard and dry as would otherwise be the case, and it is easier to keep the bowels regular.

It is certainly no matter of surprise that there should be malaise and distress, when the system is loaded with worn-out material, unfit for the functions of life, which the blood cannot remove for lack of menstruum; it is not surprising that the nervous system, which most requires regular nutrition, should suffer most; that muscles badly nourished should ache on motion; that kidneys called upon to secrete an abnormally concentrated urine should become diseased; that the highly acid urine should irritate the bladder.

This view may explain why herb teas, thoroughwort, chamomile, sage, etc., were so popular in our grandmothers' days, indeed are now popular. The bitter herb is a slight gastric tonic, but the water is a better solvent. Formerly the good housewife supplied the deficiency in drink by regular doses of herb tea; now the physician supplies it by draughts of spring water. Sometimes, in treating such patients as have been referred to, I administer a diuretic with the water, that elimination may be effected more speedily.

How much water should an adult drink in twenty-four hours? It must be taken into account that water is excreted by the lungs and skin as well as by the kidneys; also much of the food ingested contains water as one of its constituent parts. Hence the amount of liquid required as drink must vary slightly with the activity of the skin and the character of the food. If much of the diet is made up of soft solids, fruit and watery vegetables, less drink will be needed than if the diet is composed of dry meats and vegetables. The amount of soup ingested would also affect the amount of mere drink required. The average amount of urine passed in twenty-four hours by a healthy adult is stated by Dr. Flint to be about fifty-two ounces, the extremes being thirty-five and eighty-one ounces. The amount of drink necessary is stated by Dalton to be about fifty-two ounces, that is, 3.38 pints. An ordinary coffee

cup holds six or seven ounces. The equivalent of eight or nine coffee cups of drink would not then be an excessive amount. Repeatedly patients have told me that they drink only one or one and a half cups, morning and evening, and about the same at dinner, only occasionally taking soup, averaging less than six cups, sometimes small tea cups, of drink. Sometimes patients say they drink generally only a little more than a pint a day.

After one has for months and years averaged an insufficient amount of drink, and the system has become charged with used-up material, it may not be wise to immediately administer large draughts, whether of ordinary drink or of mineral water, but the quantity can be rapidly increased, and soon the normal average may be exceeded for a while with advantage.

I remember one patient, a lady, who came to me suffering from very distressing nervous symptoms not well defined, chiefly referable to the head; more a sense of unrest and disquiet in the body than distinct pain. Five months previously she had gained an idea that she must not drink much, and so restricted herself to a cup of tea night and morning, taking less than a pint of fluid in the twenty-four hours. Five weeks before applying to me the above symptoms appeared, that is, about four months only after restricting the amount of drink. This may be called an acute case.

The following case, treated at the City Hospital, Boston, is an interesting example of the condition found in these patients.

A man, 62 years old, entered the hospital, August 30th. He said he had had rheumatism at times since he was a boy, rheumatic fever seven years ago. For more than ten years he had noticed a red, sandy sediment in the vessel after micturition, which was frequent, and the quantity of urine was scanty. Seven or eight months before entrance he first noticed shortness of

breath, which troubled him in going up stairs. There was pain in the lower part of his back ; his hips ached so he could hardly step ; he had had numbness in the left leg for nearly a year. He had had jaundice two or three times and vision had been slightly indistinct. He was a large, fleshy man with a very large, tympanitic abdomen. He had a notion that he had kidney disease and dropsy and heart disease. There was nothing of the kind ; there was no cardiac lesion, no hepatic enlargement, no signs of Bright's disease. He walked as well with his eyes shut as with them open ; his gait was unsteady. He had had fainting fits, pain all over, heat in head and aching in back. No attempt was made to record all his complaints. On September 3d, 4th, 5th and 6th he passed 20, 28, 29 and 18 ounces of urine respectively. He was told to drink more freely and received fl. ext. buchu. The amount of urine rose to 40, 50, 60 and 68 ounces ; his discomfort decreased and he soon expressed himself as much relieved ; he walked much better. As there was a possibility of a syphilitic taint he was put on iodide of potassium, but not until after the improvement had commenced. During the last twenty days of his stay at the hospital he passed on an average 47.5 ounces of urine daily.

A physician applied to me for advice in regard to himself. He had many symptoms referable to overwork ; I found he drank very little and was troubled with constipation. In his case the diminished supply of fluid was not the only cause of his symptoms ; but, among other means to give relief, he was advised to drink more. In about eight months I saw him again. He was well qualified to observe the effects of remedies, and stated that the increase in amount of fluid ingested had been beneficial ; he felt better and was less constipated.

Human nature is such that if the doctor tells his patient to drink two or three pints of Cochituate or Croton water a day, in addition to his tea or coffee, he will rebel and think it a queer prescription ; but if he is told to take that amount of Poland or Allandale or some similar water, he forthwith has his keg of mineral water on tap, and drinks in faith that it will, in some mysterious way, relieve his gout, rheumatism, dyspepsia or kidney disease, or will be good for his headaches and tired brain.

Fothergill in his handbook of treatment touches upon the use of water in the way above indicated (see pp. 52, 542 and 506). It seems to me that the insufficient ingestion of water is often a predisposing or even exciting cause of many diseases; that this is more frequently the case than is usually supposed.

I find that a very large proportion of those who suffer from nervous exhaustion, neurasthenia as it is called, do not drink enough. Is it an American peculiarity to ingest so little fluid? I believe it is. Will this then partly explain the prevalence of neurasthenia in America?

I believe that one reason of success of the treatment adopted by Dr. Mitchell and advocated in "Fat and Blood," is to be found in the large amount of milk which he gives his patients.

It is not to be expected, however, that in all these cases the simple increase of fluid ingested will cure our patients. Too frequently the tissues have been so long illy nourished that that simple plan is not sufficient. The time to work the greatest cures with water is before the disease has begun.

CONGENITAL COLOBOMA OF THE EYELIDS:

(WITH A CASE.)

By DR. E. GRUENING,

SURGEON TO THE N. Y. EYE AND EAR INFIRMARY.

ON November 11, 1879, Dr. Hoexter, of this city, requested me to examine the eyes of the child, R. L. I found a healthy-looking girl, 5 weeks old, who presented a considerable coloboma in the upper lid of the left eye. The defect extended through the whole lid, dividing it into two unequal portions. The outer and larger of these portions measured horizontally 1 cm., and showed a well-formed ciliary border normally supplied with lashes and Meibomian orifices. The inner and smaller portion measured 2 mm. horizontally. Its lower border was but scantily provided with thin hairs and contained two puncta lachrymalia situated at a distance of 1 mm. from each other in a horizontal direction. A probe introduced into one punctum passed out through the other. A communication of either of the puncta with the lachrymal sac could not be demonstrated. The defect in the lid presented the shape of a trapezium, the longest line of which corresponded to the lower border of the lid and measured 12 mm. The upper boundary line of the coloboma (10 mm. in length) was formed by a fold of skin which passed gradually, *i. e.*, without any distinct demarcation, into the ocular conjunctiva. On either side the defect was bounded by the above-mentioned lid portions, which, at these points, were considerably everted and covered by hypertrophied conjunctiva. On the temporal side, the defect measured 8 mm., on the nasal, 3 mm. in height.

In all the cases of congenital coloboma of the lid, hitherto observed, Pflüger's case excepted, other malfor-

mations co-existed on the anterior surface of the globe. In my case the eye presented the following anomalies. From the inner and upper angle of the coloboma, a white band of thickened conjunctiva, 6 mm. in length, 1 mm. in width, extended, pterygium-like, over the anterior aspect of the globe to a point situated in the upper and inner peripheric portion of the cornea, causing there an opaque spot. Diametrically opposite the corneal apex of the described band, at a point in the lower and outer peripheric portion of the cornea, another white and glistening spot was found. The latter formed the apex of a triangular pouch-like swelling, the base of which lay at the outer canthus. The tumor, partly dermoid, partly lipomatous in character, belonged to that mixed form of growths denoted as subcon-

junctival lipoma (Graefe's *Arch. f. Ophthal.*, x, i, 215). In the vicinity of the cornea the dermoid nature of the tumor was especially marked, its surface being studded with a number of minute elevations and thin unpigmented hairs. Near the outer canthus the tumor showed a smooth and shining surface and a bright yellow coloration. The interior of the left eye was perfectly normal, its refraction hypermetropic. The right eye was well formed in every respect. In addition to the anomalies presented by the left eye, there existed a malformation, an excess of development, in connection with the left ear. In front of the tragus, at a distance of 2 mm., there were found three au-

ricular appendages arranged in a vertical row. The largest appendage measured 8 mm. and was pedunculated, the others were smaller and had a broad base.

It was deemed important to correct the deformity in the lid, not only in order to improve the appearance of the child but also in order to protect the eye, the cornea of which was exposed at all times whether the child was awake or asleep. The operation was performed in the following manner: The edges of the coloboma were transfixed and pared with a Graefe's knife, and after enlarging the palpebral fissure by external canthotomy, the inner and outer portions of the upper lid were rendered more movable by subcutaneous division. The lips of the wound were brought together and united by interrupted silk sutures. Borated cotton dipped into vaseline was then applied over the eye and secured with a pressure bandage. Union by first intention took place and the result was very satisfactory. Two months later I removed the dermoid tumor and the auricular appendages.

Congenital coloboma of the lid is of rare occurrence. The most complete tabulation of the cases hitherto observed has been furnished by Schleich (*Mittheilungen aus der Ophthalmiatrischen Klinik in Tübingen* 1880. *Herausgegeben von Dr. Albrecht Nagel, pp.* 114–129.)

There are altogether twenty-five cases on record, mine being the twenty-sixth.

Inasmuch as a full list of these cases has never appeared in English medical literature, I may be permitted to reproduce Schleich's table and to add my case.

1. MAYOR. Coloboma of the upper lid. Congenital fold extending from the apex of the coloboma to the margin of the cornea. (*Thèse sur quelques maladies congénitales des yeux.* Montpelier, 1808.)

2. BEER. Coloboma of the left upper lid observed in a newborn boy. Conical cornea. (*Beer. Das Auge.* Wien., 1813, p. 55.)

3. Heyfelder. Coloboma of the left upper lid of a boy 3 months old. Flattening of the upper segment of the globe. Left-sided hair lip and corresponding furrow in the alveolar process of the superior maxilla. (*Zeitschrift für Ophthalmologie* Von Ammon, T. L., 1831, p. 480.)

4, 5, 6. v. Ammon. Three cases of simple coloboma without complications. Two of the lower, one of the right upper lid. (With illustrations.) (v. Ammon. *Klinische Darstellungen der Krankheiten und Bildungsfehler des menschlichen Auges.* Berlin, 1841. Abth. III, p. 4.)

7. ——— Coloboma of the left upper lid of a man 20 years of age. Flattening and oblong shape of the cornea. Microphthalmos. Absence of the second metatarsal bone on both sides. (v. Ammon. *Zeitschrift für Ophthalmologie.* Bd. V, 1847, p. 83.)

8. ——— Boy, 6 years old. Coloboma of the right upper lid. Scleral tumors on both eyes. (Illustration.) (v. Ammon. *Journal der Chirurgie und Augenheilkunde.* B. XXXI, 1, p. 96, 1843.)

9. Cunier. V-shaped coloboma in the centre of the right lower lid. Ptosis palpebrae sup. Boy, 3 years old. (Operation. Illustration.) (*Annales d'Oculistique.* T. VII, 1842, p. 10.

10. v. Graefe. Boy, 6 months old, with complicated coloboma of the left upper and lower lids. Hair lip on left side and abnormally high arching of the palate. Coloboma of left ala nasi. Lower punctum lachrymale on the outer side of coloboma. (*Arch. für Ophthalmol.* Bd. IV, Abth. 2, p. 269.)

11. Becker. Girl, 18 yrs. of age. Congenital double coloboma of the left upper lid. Sub-conjunctival lipomata, hairs on the conjunctiva bulbi of both eyes. (Operation successful. Illustration.) (O. Becker. *Wiener Mediz. Wochenschrift, Spitalzeitung* 1863. Nos. 16–18.)

12. Horner. Double coloboma of the right upper lid. Dermoid tumors. (Illustration.) (*Klinische Monnatsblätter.* Bd. II, p. 190, 1864.)

13. Manz. Peculiar malformation on both upper lids of a man, 18 yrs. old. Folds of skin extend from the opaque corneae of both eyes to the congenital colobomata of both sides. (Illustration.) (*Arch. für Ophthalmologie.* Bd. XIV, Abth. 2, p. 145, 1868.)

14. Wecker. Coloboma of the right upper lid of a man, 30 years of age. Dermoid tumor on the inner corneal margin, cor-

responding to the defect in the lid. (Illustration.) (*Arch. f. Augen und Ohrenheilkunde.* Bd. I, p. 126, 1869.)

15. SEELY. Coloboma on the inner portion of the lower lid. Attachment of portion of lid to globe. In the upper lid a slight notch. (*Transactions of the Amer. Ophth. Society*, 1871, p. 142.)

16. STEFFAN. Coloboma of upper lid with dermoid tumor on the inner corneal margin. (*Bericht über die Augenheilanstalt*, 1872–1873.)

17. PFLÜGER. Coloboma at the the inner portions of left upper and lower lids of a boy, 1 year old. The upper canaliculus represented by a furrow leading into a cul-de-sac. Hair lip. Abnormal arching of the palate. (Operation successful.) (*Klinische Monatsblätter f. Augenheilkunde.* Bd. X, p. 250, 1872.)

18. WILKINSON. Girl, 3 weeks old. Coloboma of upper lid on either side. Membrane resembling conjunctiva, covering cornea. (*Transactions of the Patholog. Society of London*, 1872, p. 214.)

19. GILETTE. Boy, 6 years old. Coloboma of both upper lids. Absence of right upper punctum. (*L'Union Médicale*, 1873. No. 60.)

20. STREATFIELD. Woman, 40 years of age. Coloboma of upper lid on either side. Right, double coloboma with tongue-like middle-piece. (Illustration.) (*Ophthalm. Hosp. Rep.*, 1873, vol. VII, p. 451.)

21. TALKO. Double coloboma of upper lid with division of tarsus into three parts. Dermoid tumor on the upper lid. Globe normal. Man, 28 years of age. (Illustration.) (*Klinische Monatsblätter f. Augenheilkunde*, Bd. xiii, 1875, pp. 202 and 206.)

22. ——— Coloboma at the inner portion of the lower lid of a girl 20 years of age. Dermoid tumor at the inner and lower corneal margin. Anomalies of the pupil.

23. DAVIDSON. Girl, 3 weeks old, with coloboma of the upper lid. Membrane-like, hairless thickening at the outer portion of the cornea. Conjunctival fold stretching from the apex of the coloboma to the corneo-scleral margin. (Operation. Illustration.) (*Medical Times and Gazette*, vol. 50, p. 169, 1875.)

24. KRASKE. Girl, 13 years old. Coloboma at the inner part of the right lower lid. Punctum on the inner side of coloboma. Connection of the coloboma with a hare-lip by means of a cicatricial band. Trace of a furrow between the superior maxilla and the inter-maxillary bone. Fistulous canal in superior maxilla. (Illustration.) (*Arch. f. Klinische Chirurgie*, Bd. xx, 1877, p. 396.)

25. SCHLEICH. Coloboma of upper lid on either side. Well pronounced on left, less pronounced on right side. Pterygium-like formation on cornea. Tumors of lid. Peculiar streaks of hair growth. (Illustration.) (*Mittheilungen aus der Ophthalmiatrischen Klinik in Tübingen. Heransgegeben von* Dr. ALBRECHT NAGEL, 1 Heft, pp. 114–129.)

26. GRUENING. Girl, 5 weeks old. Coloboma of left upper lid. Pterygium-like band extending from the inner and upper angle of the coloboma to the sclero-corneal margin. Dermoid tumor passing into sub-conjunctival lipoma on outer side of globe. Three auricular appendages in front of the left tragus. Operation successful. (Illustrated in the present article.)

In connection with their cases, Manz, Wecker and Kraske published their views as to the possible origin of coloboma of the lids.

Manz thinks that, owing to an abnormal histological alteration in the substance originally connecting the anterior surface of the globe with the common integument, the formation of a complete upper lid is rendered impossible.

Wecker comes to the conclusion that the origin of the coloboma is to be sought in the fact that at a certain period of fœtal life the cutis covering the eyeball fails to be transformed into conjunctival tissue. He finds the proof of this assertion in those cases of coloboma which are complicated with congenital malformation on the anterior surface of the eyeball.

Kraske, who describes a case of coloboma of the lower lid, is of opinion that this malformation constitutes a true arrest of development, inasmuch as the intra-uterine fusion of the naso-ocular furrow does not take place.

According to Manz, Kraske's hypothesis is plausible, but fails to account for cases of coloboma of the upper lids.

Among the 26 cases tabulated, we find 18 in connection with the upper lids.

If we discard the hypothesis of either Wecker or Manz,

the origin of the vast majority of the recorded cases remains unexplained.

In a recent article, entitled "Contributions to the Knowledge of the Development of the Eye,"* Dr. Th. v. Ewetsky considers the question of the formation of palpebral coloboma in connection with the development of the lids.

According to Ewetsky, an epithelial membrane begins to grow from the edges of the lids at a time when they are still wide apart. The rapid development of the epithelium draws the lids together over the anterior surface of the globe. The lids are lengthened, not by any development in themselves, but passively by being drawn out by the epithelial membrane, which grows from their margin over the cornea. They approach each other, and finally become confluent along their whole margin. Ewetsky thinks that in this process of closing the lids there are periods at which a coloboma may be formed readily if any obstacle should arise in the regular development of the lid sutures.

As regards the formation of coloboma of the upper lid, Ewetsky's studies furnish a hypothesis more satisfactory than that of either Wecker or Manz, and if we adopt Kraske's views in regard to the formation of coloboma of the lower lid, we are in the possession of two very acceptable hypotheses concerning the origin of coloboma of both the lower and upper lids.

* KNAPP and HIRSCHBERG'S *Archives of Ophthalmology*, vol. 8, No. 4, p. 429.

II.

ON THE USE OF THE COLD PACK FOLLOWED BY MASSAGE IN THE TREATMENT OF ANÆMIA.

By MARY PUTNAM JACOBI, M.D. and VICTORIA A. WHITE, M.D.*

IN the clinical cases whose history has been related in the preceding number of the ARCHIVES, we studied the effect upon the urine of the cold pack followed by massage. We showed:

1. That the urine formed during this double procedure, and collected immediately afterward (the bladder having been emptied immediately before), was considerably increased in quantity, *i. e.*, the amount of water was greatly increased.

2. The amount of urea per hour was also absolutely increased, often doubled, but owing to the large excess of water, the percentage of urea in the urine was diminished.

3. The amount of extractive and of inorganic salts was generally increased, but in much smaller proportion than was the case with the urea. In not a few instances the amount of extractive was lessened.

4. In what seemed to be the typical cases of the action of the cold pack, the amount of water and of urea eliminated per hour during the rest of the day on which the pack was taken, fell below not only the amount eliminated during the pack, but below the average per hour of days preceding treatment, or days on which the pack was

* Continued from Vol. iii, No. 3, p. 296.

not taken. Thus the total amount for the day was not changed by the treatment.

In some cases the urine was not examined comparatively for the hours of the treatment and for the rest of the day, and in these a total increase of urea for twenty-four hours was often observed. But if this persisted two or three days, symptoms of *malaise* or exhaustion occurred; sometimes the patient was chilly, sometimes suffered from severe headache, in all cases from a sense of great fatigue.

From the foregoing it would appear that whatever modifications of nutrition were effected during the pack and massage, were followed by a sort of movement of compensation in the opposite direction; and this movement of compensation seemed to be an essential part of the therapeutical effect produced.

5. On a few occasions, massage was given for an hour without being preceded by any pack. The urine eliminated during this hour showed an increase of water and of urea—less marked than was usually the case when the pack had also been given. In no case, however, did an opportunity occur to test the effect of the pack entirely separate from that of the massage. As it was important to ascertain this, if only to confirm or control the experiments that have been made by others in regard to the same question, the pack was given to three healthy women, with the observance of the same precautions as in the other cases. The results are recorded in Table VII.

From this table, it is evident that the pack alone, apart from any form of muscular exercise, is capable of producing all the modifications of the urine already described; increase of water, of urea, and other solids. Moreover, as in the cases earlier described, during the hours following the pack, a movement of compensation occurs, in virtue of which, the elimination of both the fluids and the solids of

the urine falls as much below the average, as it had risen above it during the hours of the pack.

On two of the persons upon whom this experiment was made, no conscious effect was produced. The third, a

TABLE VII.

M'ch	No. I.	Amount urine in c. c.	Solids in grms.	Inorganic	Urea	Other organic	Per cent. urea	Per hour urea	Per hour inorganic	Per hour organic	Per hour amount
16th	24 hours . .	782.386			26.601		3.4	1.108			32.599
17th	24 hours . .	1062.864			28.696		2.7	1.195			44.286
18th	2 hours cold pack+¼ hour	310.00			3.62	1.	1.2	1.608			137.77
18th	22 hrs. before and after pack	472.384			14.643		3.1	0.665			21.472

April	No. II.	Amt. urine in oz.									Oz.
23d	24 hours . .	28.	48.153	10.746	29.760	7.647		1.24	0.447	0.318	1.16
24th	24 hours . .	26.50	43.751	10.562	26.601	6.588		1.108	0.440	0.274	1.10
25th	Before pack 4 hours . .	6.	9.905	2.834	5.491	1.580		1.372	0.708	0.395	1.50
	2 hrs. blanket pack . . .	3.50	6.019	1.601	3.203	1.215		1.601	0.800	0.607	1.75
	18 hours after pack . . .	37.50	40.449	10.746	22.320	7.383		1.240	0.597	0.410	2.08

April	No. III.										
24th	24 hours . .	44.	48.428	14.289	28.579	5.560		1.19	0.595	0.231	1.83
25th	Before pack 8 hours . .	14.	21.721	4.143	10.772	6.806		1.346	0.517	0.850	1.75
	2 hours cold pack . . .	14.	10.619	1.243	5.386	3.99		2.693	0.621	1.995	7.
	14 hours after pack . . .	28.50	26.505	6.731	15.145	4.629		1.081	0.480	0.330	2.

rather anæmic woman, felt extremely fatigued, "used up," during the remainder of the day. The question now arises: Can these modifications of the urine, so unquestionably shown to have been effected by the treatment, be accepted

as an index to and measure of its beneficial effect on the patients? Can they, further, in any way furnish the explanation of such benefit?

The benefits derived may be summed up as follows:

1. Increase of appetite was one of the first results observed, and this sometimes when no other benefit was derived. In the two "neurasthenic" cases, where nervous headache was a prominent symptom, the appetite diminished rather than improved. In these same cases, as already seen (see preceding paper), very little improvement in health was obtained from three and from six weeks' treatment.

2. During a variable number of hours following the pack, the patient generally experienced a feeling of fatigue of various degrees of intensity, and by no means proportioned to the degree of debility existing previous to treatment. When this fatigue lasted all day, the pack was only given every other day; and, on the intermediate days, the patient, in the successful cases, felt decidedly invigorated.

3. In several cases where sleeplessness had been a distressing symptom, it rapidly disappeared after beginning the treatment.

4. As already stated, circumstances prevented us from measuring the blood of the patients by means of the hæmitameter. But that a decided enrichment of the blood must have been effected, is shown by the reëstablishment of menstruation in three cases of prolonged amenorrhœa, with coincident amelioration and final disappearance of intense dyspeptic symptoms.*

5. In one case, a rapid involution of a subinvoluted uterus was initiated during the hydrotherapic treatment. But as ergot was freely administered at the same time, it is

* Among these we do not include the constipation, so remarkably obstinate in two of the cases; because this condition was especially treated by the large cold enemata, with the addition, in one case, of belladonna, in the other, of ipecac.

impossible to ascribe the satisfactory result entirely to the packs. But comparison, however, with numerous cases of uterine subinvolution treated exclusively by ergot, would lead us to believe that the rapidity with which the favorable result was brought about in this one, and certainly the rapid improvement in the nutrition, strength, and conscious feelings of well-being in the patient, were largely to be attributed to the packs and massage.

The question again presents itself: By what mechanism can we presume this treatment to have been influential in determining these various effects, all belonging to the one more general result, namely, improved nutrition of the blood and tissues?

The effects of the cold pack have been, by various observers, more or less accurately estimated on the pulse, the respiration, the temperature, the cutaneous nerves and certain functions regulated by them; finally, in general terms on the nutritive metamorphosis or "*stoffwechsel.*"

Concentrating our attention upon one portion only of the phenomena attending the administration of the cold pack, we have made no precise personal researches in regard to the rest, and must supplement our observations by the experimental results which have been obtained by others. We must consider separately the initial effect of the pack, *i. e.*, the sudden application of cold, and the secondary effect—when the sheet has become warm.

1.—*The Pulse.* Winternitz, repeating, with graphic apparatus, upon human beings, the experiments made by Rohrig upon rabbits, arrived at a similar result: namely, that immediately after the application of cold to the skin the heart's action is temporarily accelerated; but in from three to ten minutes, the application continuing, the pulse becomes retarded. This, of course, is meant for such non-febrile conditions which we are alone considering. The temporary acceleration is ascribed to stimulation of the

accelerator nerves springing from the cervical cord: the more permanent slackening of the pulse,* to stimulation of the vagus,—when the impression shall have been transmitted as far as the medulla.

According to Kolmann (*British Med. Journal*, 1873), the pulse of a healthy man, which by a walk had been accelerated to 104, sank to 84 very shortly after entering the pack, and in an hour was at 60. Several times was observed a fall from 72 to 44. (Quoted by Winternitz.) A fall of the pulse in the pack was uniformly observed by us whenever looked for; fall proportioned with previous rapidity.

2.—*Respiration.* The first contact with the cold sheet, as every other sudden application of cold, provokes one or more deep, gasping inspirations, followed by a brief period of rapid respiration. After this the respiration becomes slower, but not in proportion to the pulse. "There are always fewer cardiac contractions to each respiration; and the blood, therefore, remains longer in contact with the air in the lungs, with a possibility of becoming more highly oxidized."—Winternitz.

3.—*Cutaneous nerves.* The shock of the cold sheet produces the same stimulating impression on the cutaneous nerves as is caused by the sudden application of cold to the skin in any other form. The stimulating impression is necessarily transmitted to various nerve centres, upon which it acts, and from which it is reflected along motor and vasomotor tracts according to well-known laws. In this part of its action, the cold sheet entirely resembles the cold shower bath or douche, but is less powerful, because the stimulating impression of cold is unaccompanied by the stimulus derived from the mechanical impact of the water.

4.—*Cutaneous blood-vessels.* These invariably contract under the first impression of cold. The contraction seems

* Die Hydrotherapie, 1877.

effected by means of local stimulation of the vaso-motor nerves, and of the muscular fibre of the blood-vessels, without the intervention of the vaso-motor centres. For, when there is normal reaction, the blood-vessels rapidly dilate,—more rapidly than would be the case as the result of vaso-motor paresis. The dilatation coincides with the first consciousness of nervous stimulus from the impression of cold; coincides, therefore, with the excitation of the cerebro-spinal nerves of the skin; is proportioned, both in rapidity and extent, to the susceptibility to stimulus possessed by the cerebro-spinal system, and may be considered, therefore, as an active dilatation effected under the influence of the cerebro-spinal nerves. In these respects, again, the initial effect of the cold sheet resembles the total effect of the shower bath or douche.

The effect on the pulse and respiration, however, is not identical with that of the shower bath, because it covers not only a period of acceleration of both, but a period of retard as well, whereas, throughout the entire time that a douche may with propriety be administered, the pulse and respiration remain accelerated. For a therapeutic influence on nutrition, it is the secondary retard which is of value, and this is perhaps the main reason why even the initial effect of the cold pack is more valuable in anæmia and states of denutrition than is the shower bath.* While in some cases of torpid anæmia the pulse may be slow, it is well known that in the majority of cases it is rapid and feeble; at all events, extremely variable at different times. In chloro-anæmia the typical pulse has been described as ample, the blood-vessels being well filled, though with watery blood, while the tension is extremely low, so that the percussion stroke of the sphygmographic trace is ab-

* In many neurotic conditions, unaccompanied by marked anæmia, the douche, on the contrary, is much more valuable than the pack, and in our short series of cases, we have seen that the latter seemed to fail precisely in proportion to the predominance of the neurotic element.

normally high. This hydræmic pulse is certainly by no means always present, even in typical cases of chloro-anæmia, and its character would be more likely to be changed, if at all, by other effects of the cold pack, to be considered later. The retard of the pulse determined near the outset of the application of the wet sheet, is chiefly of value—and of great value—when a quick, feeble pulse indicates that the blood passes at once too rapidly and in too small quantity through the tissues to properly nourish them. "The irritable weakness," or nervous irritation, so characteristic of anæmic patients, is largely dependent upon this condition, especially as it exists in the nervous centres and in nervo-muscular tissues. All the facts known in this connection, indicate that an imperfect supply of nutrient fluid to those tissues tends to prolong their functional activity in proportion as it interferes with their nutrition. Thus, insomnia; thus, anæmic cramp of many muscles. The imperfection in the supply may depend on the quality of the blood, or on the quantity delivered to the tissue in a given time. It is this latter imperfection which alone concerns us for the moment, for it is evident that the quantity of blood in the circulation remaining the same, the duration of its contact with the elements of tissues will be less when the rapidity of the circulation is greater.

Setting aside then the condition of the quality of the blood, and the functional irritation that has been proved to depend on its deterioration, it is not difficult to show that, even without such deterioration, rapidity of circulation favors functional activity and diminishes nutrition. The famous experiments on the circulation of glands, the contrast between the state of the circulation of the brain during sleep and wakefulness, the intellectual excitement coinciding with the acceleration of the pulse under alcohol, belladonna and hyoscyamus, and the intellectual torpor, accompanied often by evidences of improved nutrition,

when, under the same drugs, the pulse is slackened, either secondarily or primarily,—these and many other familiar facts suffice to establish the proposition.

Ranke has formulated the law: only fatigued tissues can be thoroughly nourished. Because in them alone (nervo-muscular tissues at least) the habitual alkaline reaction is exchanged for acidity, as acid "fatigue-products" accumulate during the activity of the tissue.

This acid reaction increases greatly the facility with which nutritive material can diffuse from the alkaline blood* into the elements of the tissue. In proportion as the nutritive material diffuses into the nerve or muscle cell, the acid fatigue products must diffuse out. An inadequate blood stream, which would bring an imperfect supply of nutriment, would also imperfectly remove this acid detritus. Ranke's very precise experiments have shown that, while a higher degree of acidity completely depresses the functional activity of a nerve or muscle, a slight degree of acidity excites this. When the fatigue-products are imperfectly washed away, just such a slight, exciting degree of acidity may be supposed to be maintained. By unduly prolonging the tonic contraction of the cell protoplasm, this permanent excitement opposes the penetration of nutrient material (see note *infra*). While, on the other hand, this *exciting* degree of acidity is far less favorable to the nutrient endosmosis than the acid saturation resulting from complete exertion, and accompanying complete fatigue and repose of the nerve or muscle cell. Thus re-

* *Die Lebensbedingungen der Nerven*, Leipzig, 1868. "The process of nutritive assimilation and disassimilation is always active, depending ultimately on the vital properties of the tissues. In all cells (necessarily) possessed of vital contractility, one may consider, exists a certain slight contraction, or tonus, of the cell contents. Since these are inseparable from the cell membrane, therefore must this elastic membrane sustain a certain traction. Let us assume that there are pores piercing this membrane perpendicularly, then, on account of the internal traction must these be funnel-shaped and closed at the lower extremity. If now the vital energy of the cell be paralyzed, the traction is lessened, the pores are opened, and fluids pass into the cell. The pores will remain open until closed by increased internal pressure from constantly growing mass of cell contents." Ranke, loc. cit., p. 87.

sults incomplete functional activity in the cell, owing to an inadequate reserve supply of material from which to elaborate force; incomplete formation of acids; incomplete removal of acids; hence imperfect nutrient osmosis from alkaline blood, and prolonged excitation of the functional activity of cell. The first step in this series precedes the conditions of the circulation we are considering; often, indeed, constitutes the congenital defect, upon which depend so many obstinate anæmias. But the other conditions may all be produced or removed temporarily by changes in the pulse, and consequent changes in the volume of the blood stream bathing the cell.

During the initial or shock period of the cold pack, the contraction of the peripheric blood-vessels raises the tension of the vascular system, so that blood streams through the muscles, nerve centres, and viscera, not only more slowly, but under a higher pressure; second condition favoring osmosis.

The rapid, shallow breathing of anæmic persons, like the rapid pulse, is said to be "an effort of nature" to compensate imperfection of oxidation due to the deglobulization of the blood by means of more frequent inspirations of air; and is directly due to irritation of the inspiratory centre by a blood poor in oxygen. The accelerated respiration following the deep inspiratory effort determined by the shock of cold, differs from the acceleration of anæmia, in that the inspirations remain more profound, and during the few minutes that this period lasts, more oxygen is taken into the lungs, and, whatever amount of hæmoglobine is in the blood, is enabled to become more nearly saturated. The subsequent slowing of the respiration may be in part due to this increased oxygenation of the blood, which, though lasting such a brief time, may suffice to lessen the irritability of the inspiratory centre; it is in part caused by the recumbent position. According to Rosenthal,* retard in

* Die Athembewegungen, 1862.

the inspiratory movements implies an increase in the resistance habitually generated in the respiratory centre together with the inspiratory force, and intermittently overcoming the latter. When there is more resistance, a large amount of force has to be generated in the inspiratory centre to overcome it; hence delay in the occurrence of an inspiration; which, however, is only more forcible when it comes. The respiration, during the first period of the pack, is deeper because slower, other circumstances remaining the same, and the increased facility for oxygenation, once initiated, is continued.

Stimulus of the cutaneous nerves may, in regard to its effect upon the nutrition, be variously estimated. Winternitz asserts that it directly increases the nutritive metamorphosis (stoffwechsel).

It is established (Liebermeister,* Jürgensen, Riegel†) that thermic excitation of the sensitive cutaneous nerves determines an increased production of heat in the muscles;‡ the abstraction of heat by the contact of cold being only effected in proportion to the secondary dilatation of the peripheric blood-vessels, permitting exposure to cold of a larger amount of blood (Riegel, Winternitz). There is no proof, however, that chemical changes in other tissues, or other than those involved in this production of heat, are determined by the initial shock of the cold sheet. Nor, as already noticed, do these take place in persons too debilitated to respond to the cutaneous excitation. These are chilled and remain chilly for a double reason. The direct effect of the cold is felt, not only on the skin, from which blood should be driven to the muscles, but on the superficial layer of muscles as well, which are similarly anæmiated instead of becoming flushed with blood. And, at the

* Theorie der Fieber.

† *Deutshes Archiv. f. klin. med.*

‡ Lersch, Pratktische Balneologie. Zuntz & Rohrig, *Pflüger's Archiv.*, 1876 (quoted by Foster). Samuel, Allgemeine Pathologie.

same time, the sensitive nerves fail to convey to the central nervous organs such impressions as are required to stimulate the production of heat. Such persons are frequently unable to bear the shower bath, failing to grow warm, and only becoming excessively fatigued and "nervous," by the frictions employed to establish reaction. We have seen several such cases.

The increased production of heat, determined by stimulation of the heat-regulating apparatus, irrespective of the amount of heat abstracted by the cold,* involves increased functional activity: 1st. In the sensitive afferent nerves. 2d. In one or more parts of the nerve centres. 3d. In centrifugal nerve fibres of some kind terminating in muscles. 4th. In the muscles, where are performed the chemical processes involved in the production of the excess of heat.† Thus the organism is induced to perform a definite, and no inconsiderable amount of work. On this account its nervo-muscular tissues,—or a large portion of them,—are brought into a condition favorable to nutritive assimilation.

The contraction of the cutaneous blood-vessels finally, and their subsequent dilatation even before the initial effect of the sheet has passed away, are both of importance in several ways. During the period of contraction, heat accumulates in the body from diminished radiation; the muscles are irrigated with an extra supply of blood, facilitating the extra production of heat necessitated in them; the tension of the vascular system is raised, and, as already seen, as soon as the pulse slackens, the blood, circulating more slowly, and under increased pressure through the

* The provision for rapidly diminishing any excess of heat which may be thus produced, renders unnecessary a precise adjustment of the production of heat to its loss; and there is no proof that any such precise adjustment exists. The oscillations of gain and loss continue until the equilibrium of the normal standard is regained.

† The *normal* temperature of the body is probably the result of the algebraic sum of chemical processes taking place in all tissues; some of which produce, others absorb heat.

nerve centres, muscles and viscera, offers more favorable conditions for nutritive absorption. Nor is this all. Writers who have estimated the effect of the cold pack or cold bath merely in its relations to the temperature of the body, have dwelt principally on the retreat of blood from the skin to the muscles during the contraction of the cutaneous blood-vessels. There can be no doubt, however, that the retreat of blood from the surface goes much farther—reaches the viscera, especially the abdominal viscera, which are liable to become engorged under the influence of a chill. For the purpose we are considering, it is this effect that is especially important. If the hyperæmia of any of the abdominal viscera already exist, this new engorgement with blood may become a great inconvenience, as was shown in one of our cases, where the patient suffered from chronic membranous colitis. It may even be a source of danger, when the pack is given too near to a physiologic period of abdominal plethora, as digestion or menstruation; or even more so when there exists a lurking peritonitis. In a case submitted to treatment since the publication of our first article, and where, from some misunderstanding between the patient and the assistant, the pack was unduly prolonged in spite of absence of reaction, a slight pelvic peritonitis was the immediate consequence.

But when the reaction is normal, the column of blood which sets inward toward the chylopoietic organs very soon turns outward again, accelerating the entire circulation of these organs in the same direction. That is to say, the stagnation of blood, so frequent in anæmic persons in the mucous membrane and submucous tissue of the stomach and intestines, often keeping up a chronic catarrh, and constantly interfering with primary absorption from the alimentary canal, tends to become quickened into normal circulation by the return current of blood toward the sur-

face of the body, as the cutaneous vessels dilate. As a most important result, more nutriment is carried into the general circulation, and, with the abatement of the gastro-intestinal hyperæmia, the appetite revives.

The effects of the cold pack just enumerated, and belonging to the first five, ten, or fifteen minutes of its application, are, as we have several times noticed, common to it and some other applications of cold, as the douche or plunge-bath. Beneficial as they are, their advantages are much limited by the necessary brevity of their duration. The stimulation of the nervous system may indeed persist for some time after the cessation of the bath; indeed, in healthy persons habitually does so; does so in patients hysterical or "neurasthenic" out of proportion to their anæmia. For all such persons, therefore, the plunge-bath, shower-bath, or spinal douche is preferable to the pack. But in several of our successful cases, and in some others not published, the patient had long been in the habit of taking a daily plunge or shower-bath, without ever experiencing from it the peculiar benefit derived from the packs. It remains, therefore, for us to consider what physiological effects are produced by the prolonged application of the packs in addition to those characteristic of their beginning period; further, what conditions of the anæmic state are likely to be favorably impressed by such influences?

The slackened respiration, slackened pulse, and dilatation of th ecutaneous blood-vessels, already begun during the in itial period, continue throughout the pack,—at least, unless this is prolonged sufficiently to induce sweating. In this case the pulse becomes accelerated again. Sweating was never induced in the cases we have recorded; indeed, we have never seen it with a pack of two hours' duration, except in one case not included among those analyzed. With a dilatation of superficial blood-vessels insufficient to perceptibly redden the skin, but sufficient to give a feeling of

warmth and render the skin easily susceptible to redden by friction, the originally high arterial tension is exchanged for a lowered tension; cooling of the body from the dilated blood-vessels of the skin is prevented by the blankets which closely envelope the patient; even normal radiation is thus prevented, body heat is retained, the moisture of the wet sheet is warmed, and the patient passes gradually from a cold to a warm vapor-bath. In this the blood-vessels and the muscular fibres of the skin become more and more relaxed; the relaxation extends to all the muscles; a direct sedative influence is exerted upon the peripheric nerves. In addition to these obvious effects, Wintermitz justly calls attention to the condition of the nerve centres, affected partly by transmission from the periphery of the sedative impression, partly by diminution in their vascular supply. According to Schüller,* direct observations of the exposed brain of an animal wrapped in a moist pack, showed a narrowing of the pia blood-vessels, a sinking of the brain, and diminished excursion of its pulsations. The animal slept. In other experiments, Mosso has shown that during sleep the peripheric parts of the body increased in volume,† an effect which could only be produced by an increased afflux of blood to them, necessitating such a diminution of blood in central organs as was shown by direct observation to exist coincidently with sleep.

Sleepiness during the pack nearly always occurs in successful cases, and we have found the greatest amount of benefit to accrue when the patient was able to sleep for half an hour after completion of the pack and the massage.

If, during the first period of the pack, we may assume, for reasons already stated, that blood circulates in increased volume and under increased pressure through the nerve

* Quoted by Wintermitz.

† If I recollect aright, the same fact has been demonstrated by François Franck, in his essay on the changes of volume of organs. Travaux du Laboratoire de M. Marey, vol. ii, 1876, p. 1.

centres, and that in consequence, the acid fatigue-products which had been maintaining a permanent excitement of nerve elements, could be completely removed, the immediately subsequent diminution of blood supply, effected during the second part of the pack, cannot fail to be a great advantage. For it lowers the functional activity of the nerve tissues, that has been unduly prolonged, and brings them, therefore, into the condition which is a necessary preliminary to the beginning of nutritive assimilation. The diminution in the blood supply is not sufficient to interfere with this latter process, for it is not below the point which exists in sleep, the physiological period for nutritive assimilation in nervo-muscular tissues. Accepting Ranke's law for these tissues, "that they are only nourished when fatigued," *i. e.*, relaxed, we may see further, in the muscular relaxation induced by the warm moisture of the pack, a condition most favorable for the nutrition of the muscles.

According to the experiments of Rohrig and Guntz,* the establishment of an equilibrium between the temperature of the skin and the surrounding medium, is sufficient to diminish the peripheric nervous irritations upon which the maintenance of muscular tonus depends.

The direct cause of the tonus is the chemical process excited by the stimulus transmitted from the nerve centres, and which is identical in quality, only differing in quantity, from that which takes place during muscular contraction. It is this which is arrested by equilibrium of temperature—which is arrested, therefore, during the second stage of the pack, immediately after the exaggeration effected during the first stage. Unusually complete repose is therefore provided, after unusual excitation of one form of functional activity—that, namely, which is involved or implied in the increased production of heat.†

* Quoted by Nasse—Chemie u. Stoffwechsel d. Muskeln, 1879.

† It is admitted that the chemical processes, of whatever nature they may

We say one form of functional activity, by a certain license of expression,—referring in part to the chemical processes which antedate both muscular contraction and the production of heat in the muscle,—and which, as we have seen there is so much reason to believe, are identical in the two cases. The fact of cardinal importance, and so well demonstrated, that during muscular contraction the elimination of carbonic acid from the muscle is increased, while the absorption of oxygen remains the same (Ranke, Hermann), shows that carbo-hydrates are oxidized at the expense of oxygen already contained in the muscle. This oxygen is certainly not free, but is an element of some complex molecule, probably albuminous, whose decomposition is effected, like that of other albuminoids, by means of a ferment, which becomes active under appropriate nervous stimulation of the muscle. Guided by this celebrated hypothesis* (which does scarcely more than combine and formulate undoubted facts), we may affirm that when heat has been produced by the impression of cold upon the skin, the above series of changes is initiated in the muscles, beginning with the decomposition of the (probably) albuminous molecule contained in the muscular

really be, which take place in the muscle during its contraction, do not differ qualitatively from those which are constantly going on in the muscle at rest. The latter are the source of a large part at least of the constant heat of the body, as the former serve to develop a temporary excess of heat, coincidently with contractile force. Since the sensible heat developed in the muscle is in inverse proportion to the external work accomplished by this contractile force (see p. 70 of text), it should follow that any agency other than muscular contraction, which is capable of stimulating the production of heat, should be effective in proportion to the coincident immobility of the muscles. Hence, the immobility necessitated by the pack should permit a maximum development of heat, in response to the thermic irritation of the cutaneous nerves by the application of cold. The massage which follows, and which is sometimes supposed to be the principal agent in warming the patient, must be much less effective for that purpose. For whatever heat may be generated by it is rapidly eliminated through the dilated blood-vessels of the skin ; and whatever muscular contraction is excited by the massage serves to consume the heat, or else to diminish its production by absorbing the force liberated by the chemical processes which are the necessary antecedents both to heat and to work.

* Hermann, Untersuch. über den Stoffwechsel in Muskel. Voit and Pettenkofer, *Zeitsch. für Biol.*, 1866. Parkes, Proceedings Royal Society, vols. xv and xvi.

juices, and ending with the formation of carbonic acid, of lactic acid, perhaps of other acids also. We do not know whether the experiment has been tried of testing the reaction of muscles during an increased production of heat, to see if it resembled that existing during contraction; but it is certainly an experiment to perform. If this (theoretically probable) acidity exist, the muscle would be brought, by the necessity of reacting to the cold sheet, into the condition most favorable for nutritive absorption from the alkaline blood, as it is during ordinary physiological fatigue. —(Ranke.)

From this it becomes evident why warmth generated by the resources of the organism, cannot be replaced by the warmth of a vapor-bath, into which the patient should be plunged at the outset. The fact, that by means of the cold pack the organism is compelled to perform work, while, simultaneously, provisions are being made both for repose and for nutritive assimilation, is certainly one of the most important peculiarities of this method of hydro-therapeutics.

The vigorous massage which, in our clinical cases, followed the cold pack, served to combine several of the effects produced at different periods of the packing. Thus, it stimulated the cutaneous nerves and dilated the superficial blood vessels to a greater extent than before, so that the skin reddened intensely as it rarely did from the pack alone. Again, the passive movements of the limbs which caused actual contraction of almost all the muscles of the body, increased the vigor of muscular circulation, and tended to obtain, in proportion to the permanence of this effect, its known results in increased production of heat, in muscular nutrition, in the disappearance of chronic muscular fatigue, in relief to abdominal plethora.

Judging merely by appearances, an observer would prob-

ably be ready to ascribe much greater influence in these respects to massage than to the cold pack. Weir Mitchell's popular little essay on "Fat and Blood," has recently familiarized the public with a view of the nutritive benefits to be derived from pure massage, which, in our opinion and according to our experience, are rather exaggerated. Dr. Mitchell speaks of a muscle as "a species of throbbing heart," whose contractility can be excited mechanically merely by friction and pinching from the surface of the body. That some effect upon the circulation may be thus produced cannot be denied; the frequent success of massage in removing fatigue, aching, pain and lameness from fixed positions, or even from sprains, testifies to this possibility. Passive movements of the limbs at each joint, which, in our cases, were always combined with the friction movements, come, however, much nearer to the standard effect as obtained by voluntary contractions, than can be the case with either frictions, or slapping, or pinching of the muscle, as far as it may be reached. These manœuvres may and do cause dilatation of the blood-vessels in the muscles; they may, when sufficiently profound, cause isolated contraction of fibres, limited to the point of contact, but they can excite no general contraction of the active muscle.

In passive movements, however, where the two points of insertion are mechanically approximated to each other, and the entire muscle shortened, to be alternately extended or stretched, the characteristic effects of muscular contraction may be obtained, though to a less degree than after vigorous voluntary muscular exercise.

It is an interesting question whether the less degree of fatigue experienced by patients after massage and passive movements, as compared with that caused by voluntary motion, be due simply to the fact that in the first case the

muscular contraction is less energetic, in other words, that the quantity of exertion is less; or whether there be some qualitative difference, in virtue of which the passive motion is rendered at once more endurable to anæmic persons, and more advantageous for nutrition. We incline to the latter opinion, because simple diminution in the quantity of voluntary muscular contraction, far below the degree really induced by massage, is far from sufficing to avoid fatigue. Indeed, in the class of persons we are considering, the degree of muscular exertion involved in the act of walking, or even sitting upright, is well known to cause an intolerable and persistent uneasiness, while more active exercise is liable to excite such severe palpitations, headache and other symptoms of exhaustion that it cannot be sustained at all.

A portion of the difference between the effect of voluntary and of passive exercise, depends on the diminished work, which, with the same amount of contraction, is performed by the muscles in the latter case. As has been shown by Béclard,* and confirmed by Fick,† much more warmth remains in the muscles which contract without raising a weight external to the body, than in those which do perform this work. In the latter case, either a portion of the heat produced is converted into work, or else the production of work by the force liberated during muscular contraction, subtracts just so much from the amount of heat which can be produced by the same force. But in ordinary muscular exercise, the muscle is at least compelled to move the weight of the limb, while in passive exercise this weight is sustained by the hand of the operator. The amount of work performed is therefore reduced to a minimum; and, could the muscle be made to contract power-

* Traité de Physiologie, 1868, which re-states the experiments of the original essay.

† Quoted by Hermann. Allg. muskel-physik, in new "Handbuch der Physiologie." As shown by Hermann, Béclard's experiments are not really refuted by those of Heidenhain, still less by the counter statements of Dupuy, in *Gaz. Méd.*, 1865.

fully, the amount of heat generated and saved would be at a maximum. The contraction is less powerful than *might* be effected voluntarily; but probably more powerful than would be effected by the debilitated individual submitted to the massage. Moreover, as shown by Heidenhain's experiments, stretching of the muscle, independent of the force of contraction, accelerates the chemical changes which result in the production of heat; and stretching can be very well performed passively. But the most important cause of the toleration, without excessive fatigue, of passive exercise, lies in the preparation for muscular contraction by means of increased blood supply, effected by the preliminary frictions which should have determined a dilatation of the blood-vessels.

It is often customary to speak as if the increased blood supply in massage were the direct and constant consequence of the muscular contraction, as it is in health, and within certain limits of muscular exertion. But when a *fatigued* muscle is forced to contract, the blood supply ceases to increase; the production of heat diminishes;* indication that the chemical changes which should be effected are diminished. Whether from clogging of the muscle with acids (Ranke, Nasse), or from commencing coagulation of the myosin (Hermann), or from consumption of the (albuminous?) molecules whose fermentative "splitting" was the source of contractile power, or from a combination of these conditions, the muscle which has once fallen into a state of fatigue cannot be made to improve its circulation or its nutrition by being stimulated to fresh contractions. It will, until utterly exhausted, respond to such stimulus, but only to fall into greater exhaustion.

These considerations have been entirely overlooked by Mitchell, in the popular essay already referred to, perhaps

* Carrieu, De la Fatigue, *Thèse de l'agrégation*, 1878.

because in that essay no scientific accuracy was aimed at, but only certain rough, practical results. In this particular case, however, the theoretical inaccuracy constantly tends to defeat the practical benefit. Anæmiated muscles are in a state of chronic fatigue. For reasons already suggested, it is true that massage *may* succeed in exercising such muscles without increasing fatigue, on the contrary, while improving their nutrition by direct effect on the circulation, and not indirect, as a consequence of their contraction.

But when this is the case, we belleve it is owing, as already stated, to the dilatation of blood-vessels previously effected. When, as not unfrequently happens, the movements of massage do not secure this increased vascularization, independent of muscular contractions, the latter are often more exhausting than beneficial. It is precisely in such cases that comes into play with such advantage the peculiar mechanism by which the cold pack is enabled to flush the muscles with blood.

(*To be continued.*)

EDITORIAL DEPARTMENT.

LUNACY REFORM.

IV.

THE RIGHT OF THE INSANE TO LIBERTY.*

"The true principle to guide our practice should be this,—that no one, sane or insane, should ever be entirely deprived of his liberty, unless for his own protection or for the protection of society."

HENRY MAUDSLEY.

It is fair to say that, in the present state of psychiatry in America, to be pronounced insane by physicians, by a judge, or by a jury, means imprisonment for months, for years, or for life. To put it in another way, there is a disease which reduces its victims to a level with persons accused of crime, and exposes them to loss of liberty, property and happiness.

Is this just? Is this worthy of a country like ours, which aims to be foremost in works of philanthropy, and preëminent as regards individual liberty?

Let us review the facts of medico-legal practice in the State of New York, on which these statements rest.

1. A man has acute mania, he becomes unmanageable at home, is dangerous to himself and to others. This man is summarily placed in a carriage, taken to an asylum and locked in without any process of law. Before a certain time has elapsed after his reception (five days), certificates duly executed before a

*Read before the National Conference of Charities and Corrections at Cleveland, Ohio, on Thursday July 1, 1880.

judge of a court of record must be furnished to the superintendent of the asylum as a justification for further detention.

Viewed without prejudice, this course seems the only one which can be pursued, and consequently we deem it right. For to wait many hours or several days for all the legal forms of commitment to be executed, might endanger the life of the patient or of those about him. Still it must be remembered that such cases of very severe acute mania are rare. In most cases a delay of twenty-four hours can do no harm.

2. The same patient recovers in greater part; he becomes quiet, and, with the exception of an occasional false notion and a degree of mental enfeeblement, is like most well men. Yet by virtue of the power of the original commitment this man is kept confined to the asylum, he is yet a prisoner. If he wishes to walk, or to breathe fresh air, or to attempt some little out-door work, doors must be unlocked to let him pass, and when he returns to his room or ward he is locked in for the night. And this stage of partial recovery may last almost indefinitely. The sole power of discharge rests in the judgment and hands of a single man, the superintendent; the patient is practically under a most singular form of tyranny for any enlightened State to permit to flourish.

No matter how scientific or how good a man the superintendent may be, I hold that this degree of authority, of uncontrolled authority over the liberty of citizens, ought not to be allowed.

3. The same sort of a case, *i.e.*, one of acute mania, terminates unfavorably. The delirium is transformed into a group of more or less fixed delusions, kept up by hallucinations; the memory and judgment are fatally impaired, in other words, secondary dementia is established. If, as in innumerable cases now in asylums, the delusions of the patient are not delusions of persecution or of any other form likely to lead to an outbreak of violence or destructiveness, is it right to keep this patient in the more or less costly and elegant prisons we call asylums?

Why should not this remains of a man enjoy freedom under a certain supervision? Why should he not be allowed to go and come as he pleases and to have what little pleasure there remains for him in life?

Let us make a further supposition, which is perfectly in accord with cases daily observed. If this harmless dement have property of his own, why should not this property be used to provide him with a suitable mode of life, and expended in reasonable amount for his benefit? There is good reason to believe that many such patients are kept in asylums, private or public, indefinitely, unprovided with the luxuries or quasi-luxuries to which they are entitled. After a few years of patience on the part of the relatives, and of more or less conscious misery on the part of the patient, the disease ends in death, and the carefully preserved property of the deceased is duly apportioned among heirs.

4. A woman drifts through domestic trouble, semi-starvation, into a state of cerebral mal-nutrition and anæmia, delusions of a melancholic type appear, together with a depression and sadness which cannot be reacted against.

Is it right to imprison such a woman, to place her actually under lock and key for months?

In some cases I am willing to admit that it is justifiable, but in the majority of cases I believe that such a procedure is unjust, morally and legally, and decidedly opposed to the patient's recovery.

To decide which cases belong to the former category and which to the latter, and to judge when a given case passes from one category into the other, requires, I admit, careful observation and ripe judgment, but not more, I am prepared to maintain, than do many other problems in medicine and surgery which are decided every day in our midst with average good success.

5. Cases of primary dementia occur frequently in both middle-aged and old people. Many such patients are simply foolish, but quite harmless. Why are they sent to asylums except for safe keeping, because of the lack of relatives, or because of pecuniary inability of friends to take care of the deteriorating member of the family, or lastly, pretty often, I fear, because of unwillingness to keep the patient at home?

In all these three categories I believe that confinement under the present asylum system is unjustifiable, legally and morally.

What right have we to bar a man's window and keep him all day, or a part of the day, for months and years, under lock and key, because he is simple or foolish, or, to put it technically, demented?

The pauper insane of the harmless demented class must, I suppose, be aggregated in institutions, preferably under medical supervision if not medical control, but have we not the right to demand that these unfortunates shall have the maximum of personal liberty consistent with safety to themselves and others; that they shall be both employed and amused in an inexpensive manner? Must we not remember that so long as any reason survives, perhaps longer, the animal capacity for simple enjoyment persists? How many hundred chronic insane in this nation are quite capable of enjoying a game of cards or dominoes, bowling, a pipe of tobacco, etc., in comparative freedom, under a surveillance that need not at all resemble the present prison-like manner of watching patients in asylums?

6. Epileptic and hysteric patients are not rarely detained in asylums without justification. I have had personal knowledge of such cases.

The alleged cause of certification originally, and of unlimited detention afterward at the request of relatives, is some psychical disorder which, in a majority of cases, is temporary, and quite as much under the control of a physician as of a superintendent.

I do not, let it be clearly understood, mean to say that such cases should not be sent to institutions, but I protest most energetically against their commitment to prison-like asylums, to their forced association with the insane, and their subjection to the imperfect medical treatment which obtains in most asylums.

Removal from home often does much good in these diseases, and is a *sine qua non* to recovery in certain cases of hysteria; but from this statement to the present evil practice of judicial commitment and locking up there is a vast gulf—a gulf as deep and wide as is the great human right to personal liberty.

I do not mean to refer in detail to a last class of cases, viz.,

those in which a sane person is fraudulently imprisoned as insane under our present imperfect laws.

I firmly believe that such cases are extremely rare, and from my personal knowledge of many of the superintendents of insane asylums, I do not believe that any of those gentlemen would for one moment countenance such a procedure.

I trust that I have made myself understood.

With many others, some of them to be cited further on, I believe :

a. That a large number of the inmates of asylums could be taken care of with open doors and unbarred windows, and, of course, without restraining apparatus.

b. That many insane now confined in our asylums could be trusted almost implicitly to go and come at will, could be given nominally remunerative occupation, and, above all, could be provided with simple amusements suited to their stations in life (and I do not refer to costly stereopticons, or the solitary billiard table, or the lectures, to which superintendents now refer with so much complaisance).

c. That the phases of insanity should be watched more closely with especial reference to early discharge of a patient, to his transfer to another institution, to the amount of liberty allowed him, etc. And I do not think it safe to leave this power wholly in the hands of the superintendent.

d. That the time has come to look around and attempt in this country the English and Scotch plan of placing harmless insane persons singly in the families of farmers and of others willing to undertake the task under frequent and efficient visitation.

There is not, I dare say, a single county in the State of New York where a number of harmless chronic or even sub-acute cases of insanity might not be safely and *happily* treated or kept in families, and where a good yet just and firm country doctor could not be found to visit a dozen or more of these patients occasionally and unexpectedly, and keep a strict watch over their health and happiness, reporting fully to the State Commissioner or Commissioners of Lunacy, or to a special general inspecting official.

There are, I have reason to believe, many families who would much prefer to keep their insane in this manner, at a reasonable cost, who are made to shudder at the mere mention of an asylum, whose long and desolate corridors they see in imagination, whose locks and keys they almost think they can hear, and whose deteriorating influences upon patients they more than half suspect.

While expressing the above views I also desire to state just as clearly my belief that certain cases of insanity, more especially acute mania or melancholia, all psychoses accompanied by homicidal, suicidal or marked destructive tendencies, should be placed as quickly as the law will allow in confinement. But even here we may implore that the confinement may be made as easy as possible for the unfortunate patient, who is, contrary to public prejudice, just as much a sick man as if he had phthisis or a chronic ulcer; and that all the barbarous means of corporeal restraint, such as cribs, jackets, straps, etc., shall be radically done away with as asylum implements, as a part of the nosocomial *armamentarium*, and only applied by express directions of the medical officer in exceptional cases, very much in the same way as we resort to heroic medication or surgical procedures in general practice.

If you will pardon a simile, I would say that I look upon restraining apparatus in the treatment of insanity very much as upon the lancet in the treatment of general diseases. The sight of restraining apparatus in an asylum seems to me as demoralizing and uncalled for, as would be the carrying of a lancet and the celebrated palette by a physician of to-day in his hospital visit. Bleeding is generally condemned, and so is physical restraint of the insane, excepting in this country, and yet I will not deny but that once in a while, under peculiar circumstances, both phlebotomy and restraint are justifiable. Let us join in the hope that before many years both procedures will be equally rare.

I desire to close this essay by reference to the opinions of much abler men, and much more expert specialists than myself upon this subject. And I believe that the gentlemen whose views I am to quote are so eminent in their specialty that the most bigoted

defender of our present prison system of asylums cannot question their right to be considered as foremost authorities.

Dr. Henry Maudsley, who has held almost every possible official position in relation to insane administration in England, and who is justly regarded as one of the most distinguished European alienists, expressed himself as follows years ago. I quote from the second edition of "The Physiology and Pathology of the Mind," London, 1868.

"To be a lunatic, as public sentiment goes, is to be cut off socially from humanity. With such feeling prevalent with regard to the insane, can it be thought possible that the treatment, at present sanctioned by general approbation, should be the most just and humane possible. The feeling is one which cannot be justified, and the system which it inspires cannot be just. That system is the system of indiscriminate sequestration—of locking up a person in an asylum simply because he is mad.

"Now I believe this practice to spring out of an unjust feeling, as already said, and to be founded on false principle, as I shall now endeavor to show. The principle which guides the present practice is that an insane person, by the simple warrant of his insanity, should be shut up in an asylum, the exceptions being made of particular cases. This I hold to be an erroneous principle. The true principle to guide our practice should be this : that no one, sane or insane, should ever be entirely deprived of his liberty, unless for his own protection or for the protection of society" (pp. 494–495).

"I venture, indeed, to affirm in opposition to it, that there are many chronic and incurable insane persons, neither dangerous to themselves nor to others, who are at present confined in asylums, and who might as well be at large" (p. 495).

"Another objection to the liberation advocated will be, that the insane in private houses will not be as well cared for as they are, nor have any more comfort than they now have, in well-conducted asylums. The quarter from which this objection is urged taints it with suspicion ; I never heard it put forward but by those who are interested in the continuance of the present

state of things. Those who make it, appear to fail entirely to appreciate the strength of the passion for liberty which there is in the human breast; and as assuredly there are but very few persons who would not infinitely prefer a garret or a cellar for lodgings, with bread and water only for food, to being clothed in purple and fine linen, and faring sumptously every day as prisoners. I can well believe that all the comforts which the insane person has in his captivity, are but a miserable compensation for his entire loss of liberty, that they are petty things, which weigh not at all against the mighty suffering of a life-long imprisonment" (pp. 496–497).

"For the reasons adduced, I cannot but think that future progress in the improvement of the treatment of the insane, lies in the direction of lessening the sequestration and increasing the liberty of them" (p. 501).

"Not the least of the evils of our present monstrous asylums, is the entire impossibility of anything like individual treatment in them" (p. 502).

"Indeed, I cannot help feeling, from my experience, that one effect of asylums is to make some permanent lunatics. * * * And I can certainly call to mind more than one instance in which I thoroughly believe that the removal of a patient from an asylum was the salvation of his reason" (p. 503).

Our distinguished countryman, Dr. Samuel G. Howe, everywhere known for his philanthropic efforts, enters a noble protest against the unnecessary restraint of lunatics by confinement to asylums, in the Sixth Annual Report of the State Board of Charities of Massachusetts, for 1869. I quote:

"Yes, disguise it as we may, we do keep under unnecessary restraint and in a sort of slavery, a multitude of unfortunates who sigh for liberty, and to whom it would be very sweet. Their appearance of quiet and their seeming acquiescence are oftentimes utterly deceptive. Bewildered, doubtful of their own power of self-guidance, half conscious of insanity, still they know enough to know that the whole power of society holds them in its grip, and they resign themselves in despair" (p. lxxxvii).

"It would be folly to deny that restraint by walls, by iron sashes, by oaken doors and by constant guard is necessary for a certain class of patients under our mode of treatment. But it is equal folly to maintain that it is necessary for all, or for nine-tenths; and if not necessary, upon what ground can we defend our violation of a right which the lunatic never forfeited?

It is sinful and criminal to abridge unnecessarily the freedom of an innocent man; and it is, moreover, cowardly and cruel to abridge that of an unfortunate lunatic." (p. lxxxix.)

In the next place I quote from G. Fielding Blandford's work on "Insanity and its Treatment," London edition, 1870, American edition, 1871. He says:

"Doubtless, you have all heard of the moral treatment of insanity, but shutting a man up in an asylum can hardly be called moral treatment." (p. 379.)

"In asylums they dwelt from year to year, a few walking beyond the premises, but none sleeping beyond, or going to any places of amusement like ordinary men. Now, from all asylums patients are sent to the sea-side, to the theatre, the picture galleries, and each proprietor vies with his fellows in providing recreation and entertainment for his patients—in proving, in fact, how little they need the restraint of an asylum." (p. 383.)

[The above passage applies only to a special class of private asylums in Great Britain, but its bearing on the general question is obvious.]

"As the last generation did away with the fetters and mechanical restraint used in asylums, so let the present release from the restraint of an asylum all those capable of enjoying a larger amount of liberty and a freer atmosphere than that in which they now fret and chafe." (p. 385.)

Dr. John Charles Bucknill is known to the profession in this country as an eminent authority upon the topic in hand.

He has this spring given us in book-form the series of papers on Lunacy Law Reform which appeared in the *British Medical Journal* in 1879. The purpose of these papers is mainly warfare upon the private or proprietary asylums as they flourish in Eng-

land, but scattered throughout the volume are numerous passages proving how thoroughly Dr. Bucknill, after a most varied experience with the insane and with asylums, coincides with Dr. Maudsley in the opinion that for many cases of insanity or mental unsoundness confinement to an asylum, subjection to asylum routine and deprivation of liberty and social enjoyments are cruel and legally unjustifiable.

I quote a few sentences only :

"Custom-blindness (which is more than color-blindness, because it blurs the outline of things as they are) often leads official people to associate unsoundness of mind with detention under care and treatment as correlative if not identical conditions ; but with the general public it is not so, and still less with the medical profession, and still less again with that of the law." (pp. 25–26.)

" * * * And, therefore, it would appear to be imperative to a proper certification of a lunatic for detention in an asylum that the reasons for which such detention is needful should be fully stated upon the face of the documents, and distinguished from the facts which simply indicate lunacy." (p. 26.)

"But it is not merely the happy change which takes place in confirmed lunatics when they are judiciously removed from the dreary detention of the asylum into domestic life, it is the efficiency of the domestic treatment of lunacy during the whole course of the disease, which constitutes its greatest value, and of this the author's fullest and latest experience has convinced him that the curative influences of asylums have been vastly overrated, and that those of isolated treatment in domestic care have been greatly undervalued." (p. 114.)

"Many a suicidal patient can live as safely with two faithful and skillful attendants in a villa or cottage as in any asylum, and in the free air he will walk or drive, employ or amuse himself in various ways, and recover without the asylum brand or the asylum danger of falling more and more into subjective ways of thought and shunting into the sad list of incurables." (p. 116.)

E. C. SEGUIN.

NEW BOOKS AND INSTRUMENTS.

Transactions of the American Gynecological Society. Volume 4; for the year 1879. 8vo, pp. 506. Boston, Houghton, Mifflin & Co. (The Riverside Press, Cambridge), 1880.

No member of the profession who feels an interest in one of its most rapidly and ably developed specialties, can fail to welcome this book. It is the annual record of the most important association in America devoted to gynecology, and American gynecology is confessedly the most advanced in the world. Its dress is as attractive as faultless type and appropriate binding can make it, and its contents, if sometimes disappointing (as must candidly be acknowledged), affords in the main abundant food for thought and instruction.

Following the approved method of the Transactions of the London Obstetrical Society, the volume contains:

1. A list of the officers for the year.
2. A list of Honorary Fellows.
3. A list of Fellows in ordinary standing.
4. Proceedings of the Fourth Annual Meeting.
5. Papers read at the Fourth Annual Meeting, preceded by the annual address of the President, and followed in most cases by discussion of the subject treated.

Passing over the first of these, although the President's address is fertile in suggestions and bristles with the happy illustrations that characterize all of Dr. Thomas' writings, we propose to comment on some of the more striking papers and the subjects treated.

The first two papers (which were properly discussed together) are "Hints relative to intra-uterine medication," by Prof. James P. White, and "Intra-uterine medication by iodized phenol," by Dr. Robert Battey of Georgia. The former deals with the methods

of carrying medicaments into the uterine cavity, and eulogizes glass rods as applicators for nitric and chromic acid to the cervical canal, and glass tubes with an air bulb to convey similar fluids into the cavity of the womb. Dr. Battey advocates "iodized phenol" (a mixture of one part of iodine to four of liquified carbolic acid), which he applies either on cotton wool tents, left *in sitû*, or with a slender hard rubber probe wrapped with cotton wool.

In the discussion of these papers, in which many members who bear names of national renown took part, it is truly lamentable to find that only one—the President—felt called upon to take firm and decided ground against procedures which fill our hospitals, and the consulting rooms of gynecologists, with cases of intractable dysmenorrhœa. Both Dr. Byrne of Brooklyn, and Dr. Mundé objected casually to the extreme measures of Prof. White, but the general opinion gave no dissent from advice, for which, however distinguished its source, no reprehension seems strong enough. True, this applies to the use of mineral acids and the stronger caustics rather than to iodine; but it is equally true that all injections into the non-puerperal uterus are hazardous in the extreme. It is devoutly to be hoped that the day will soon come when both sponge-tents and intra-uterine injections for the treatment of uterine disease will be equally things of the past.

Next come two papers on "Puerperal Septicæmia," by Prof. Jenks of Chicago, and Dr. Chadwick of Boston. The object of the first, which is elaborate and ably written, is the advocacy of systematic washing out of the uterine cavity with antiseptic fluids; and here, as the writer justly observes, the objections to this practice in the non-puerperal uterus "are not applicable to its use for the prophylaxis or treatment of puerperal septicæmia." The diversity of conditions is so great as to call for no comment. The conclusions reached in both papers are the same, with the trifling difference that one author prefers carbolic acid, the other permanganate of potash. In their discussion by the Society a curious variety of opinions was expressed, one member lauding quinia in large doses, another the hypodermic use of the hydrobromate of quinine, others criticizing the use of veratrum viride as a curative (!) agent, and still others objecting to all injections as likely to do more harm than good.

The weight of opinion, however, was decidedly in favor of carefully used antiseptic injections of the cavity, preference being

given to Chamberlain's glass tubes for conducting the fluid, general support of the vital powers, and depression of the temperature by means of quinine and refrigeration of the surface.

Next in order follows a learned paper on the "Cicatrices of Pregnancy" by Dr. Busey, of Washington, and a more practical and very able essay on "Prolapse of the Ovaries" by Dr. Mundé, of New York. Mr. Spencer Wells contributes a detailed account of his first case of "Battey's Operation," and Dr. John Byrne discusses the relative merits of the thermo- and galvano-cautery in cystotomy.

Dr. Isaac E. Taylor suggests the importance of the proposition "that early (not frequent) application of the forceps in the first stage of natural labor is as necessary, safe and justifiable as in the early second stage," and supports his point in an elaborate statistical paper. Next comes an essay on "Elongation of the Cervix Uteri," in which amputation of the cervix is advised; numerous cases are narrated, but neither in the paper, nor in the discussion following it, is there a hint of the fact that many a case of imagined elongation of the cervix is merely the result of a prolapsed vaginal wall.

When we recall the disastrous effects of cicatrical contraction of the stump after this operation, and the fact that even the peritoneal cavity is not infrequently opened, (the author of the paper naïvely reports one such case), it is difficult to imagine more pernicious teaching than these few pages contain.

Want of space compels us merely to mention Dr. Johnson's admirably written paper on "Mismanaged Labor;" and a number of other essays, which will well repay perusal. We therefore come at once to the discussion of Extra Uterine Pregnancy, based upon Dr. Reeve's paper on the Electrolytic treatment of this Condition, and one of the most salient features of the volume. It is safe to say that, if the book contained nothing else, this discussion would make it well worthy of consultation by every practical gynecologist.

Nothing within this domain has advanced more rapidly from the maze of doubt and uncertainty into the light of scientific accuracy and precision; nothing is more momentous in its possibilities to the lives of those we all hold dear. Who has not known of children orphaned, and precious lives lost by the ignorance or indecision of the physician in such cases! Before death all is confusion and doubt—at the autopsy all is made clear. The almost insuperable difficulties of its early diagnosis, coupled with the

necessity of more accurate knowledge at the outset, as a basis of success in its treatment, had long been fertile themes for discussion; but, before the lucid expositions of Stephen Rogers in 1867, and of Parry in 1876, little advance was made. Now matters are happily very different; and it is not too much to say that one of the gynecologists of whom New York is justly proud, T. Gaillard Thomas, will owe no little of the enduring fame he has earned to his brilliant efforts in the elucidation and treatment of this appalling casualty.

The subject is too important for condensation, and we refer our readers to it in the original.

The volume closes with a biographical sketch (with portrait) of the late Dr. Marmaduke B. Wright, of Ohio, a list of societies devoted to gynecology, and a most valuable index of gynecological and obstetric literature for 1878. [C. C. L.]

The Students' Manual of Venereal Diseases. Being the University Lectures delivered at Charity Hospital, B. I., during the Winter Session of 1879–80. By F. R. STURGIS, M.D., etc. New York: G. P. PUTNAM'S SONS. 1880.

In this excellent little work Dr. Sturgis has avoided, to a very great extent, the discussion of theories, though he declares unhesitatingly, his assent to the dualistic doctrines of venereal ulcers. It is, indeed, surprising how little theories influence the course of correct practical observation. The book, so far as its clinical details are concerned, might have proceeded from the camp of the unicists as well as from that of the dualists. This statement the author would probably resent; but there can be no question that there is at present much less disposition to accept the clearly defined physical differences described by early authors of the dualistic school as characterizing the two kinds of sores. To-day, more stress is laid upon etiological considerations, and dualistic writers are more concerned in maintaining the existence of two varieties of venereal ulcers of essentially different nature, than to attribute to each, sharply defined characters. This book deals almost entirely with matters of practical interest, and with charming brevity and clearness.

The author wisely objects to the term "chancroid." It is a term, as used in this country, with a signification different from that intended by its originator (Clere), and at variance with the terminology adopted by most writers. Weight of authority alone retains it with us, and it is to be regretted that Dr. Sturgis, in con-

demning, did not at once discard it. The "simple venereal ulcer," as he prefers to call it, is admirably handled; indeed, but little that is valuable is left unsaid. The wise precaution, not to rely too implicitly upon the absence of induration as pathognomonic of these ulcers, is given with emphasis; but while the author points out that true syphilitic chancre may sometimes be without induration, we regret that he has not noted an occasional induration of "simple venereal ulcers," where the infiltration is decidedly more dense than the expression "œdematous and doughy" implies. We would refer, for example, to the induration sometimes found when these ulcers are situated around the preputial border, causing phymosis. The suggestions for treatment of this form of ulcer are excellent; indeed, in a few pages, we have all that is most valuable.

Passing to the "initial lesion of syphilis," the author is in accord with many writers in considering it an expression of syphilis already established, not the "initial lesion" in its true sense, but the first visible symptom. Although the question could not be properly discussed in such a book, we think it has been dismissed rather more summarily than warrantable, especially in view of recent experiences in excising chancres. But with the practical parts of the lectures upon syphilis we have no quarrel. They are excellent. We fear, however, that some readers may be misled by the description of "ulcus elevatum" as an "initial lesion" exclusively. This form of ulcer undoubtedly occurs, also, as a simple non-infecting ulcer or "chancroid."

We also disagree as to the propriety of deferring constitutional treatment until after the manifestation of secondary symptoms, because one may be in doubt as to the true nature of the disease, in consequence of the obscurity of the symptoms, etc. In very many cases there can be *no* reasonable doubt as to the inevitable result, when to delay treatment would be like waiting to attack an enemy until he is well intrenched; and if it is only "in exceptional cases" that treatment should be instituted, "because, in many cases, it is impossible to diagnosticate the nature of the lesion under investigation," we have a painful confession of the unreliability of signs upon which so much has been predicated. It would, of course, be folly to commence constitutional treatment until one is certain that the patient will have constitutional symptoms. This point settled, longer delay is unjustifiable.

The lectures upon the "syphilides" we think hardly equal to

those preceding. Sturgis' objection to the bad custom of naming these eruptions from their resemblances to various non syphilitic eruptions, such as *psoriasis* syphilitica, etc., is just, and it is unfortunate that he should have seen fit to employ so well-established a term as erythema papulatum, even though the symptom is well described thereby. Sufficient attention has not been given to early pustular syphilides, which are surely more common than represented, and the author is certainly wrong in asserting that pustules are more deeply seated than papules, necessarily. Sometimes they are, sometimes they are not, and just here we have the difference between the secondary and tertiary pustules. Most properly is the student cautioned against too great reliance in the "coppery hue" of the syphilides.

Mucous patches, syphilitic adenitis, and affections of the special organs are admirably treated of. Almost too much, however, has been sacrificed for brevity, and the same must be said of what follows upon syphilis of the bones and nervous system. This can hardly be called a fault, since the nature of the work makes it unavoidable. What is said about treatment is brief, though sufficient, bearing upon all questions likely to arise.

Gonorrhœa and its complications occupy the three last lectures, which are as satisfactory expositions of the subject as we have seen. In that very imperfectly understood malady, gonorrhœa in women, Dr. Sturgis' experience has been very fruitful, and his words are full of value.

Not the student alone will find this "Manual" useful; practitioners can find in it much more profitable reading than in most "student's manuals," and they will have the satisfaction of knowing that it contains all the essentials of a practical knowledge of venereal affections. [J. E. A.]

A Practical Treatise on Nervous Exhaustion, (Neurasthenia), Its symptoms, Nature, Sequences, Treatment. By GEORGE M. BEARD, A.M., M.D. New York: William Wood & Co. 1880. 8vo, pp. 198.

The object of this book seems to be to show the vast extent to which nervous exhaustion prevails in this country, and how little it is understood by the medical profession. It is true that no systematic treatise on neurasthenia has been written before this, but physicians have certainly recognized and been familiar with the condition for many years. The text-books, while failing to treat of nervous exhaustion separately, describe diseases which arise from this state of the nervous system.

There is no doubt that it is of value to remind medical men that neurasthenia is a real and tangible affection, and Dr. Beard has very ably and forcibly shown that conditions which are sometimes regarded as imaginary or hysterical, are actually the result of disorders of the nervous system.

On the other hand, it is of questionable advantage to put within the reach of the laity, a work on a morbid state which has already been made a *bête noire* to numbers of people by the writings of charlatans on the subject.

After reading the first few pages of this book, we began to question if it were altogether intended for the medical profession, but our doubts were set at rest when we reached page 54, and read that one symptom of neurasthenia is "coccydynia—that is, tenderness with pain and sometimes severe neuralgia at the tip of the spine, what is called the coccyx." This definition is surely not necessary for even a student of medicine. Again, on page 153, when speaking of the use of the bromides, the author recommends large doses, and adds "they are not to be used in this way domestically."

Seventy-five pages are devoted to the symptoms, and forty-six more to the nature and diagnosis and prognosis and sequences of neurasthenia. The symptoms are fully and clearly defined, but there are many conditions accredited to this disease that in our opinion do not belong to it. Idiocyncracies of all kinds are put down as belonging to neurasthenia. If a person is timid in the dark, and has been so from childhood up, he is said to have nervous exhaustion. A man who has a desire for strong drink has merely a symptom of neurasthenia.

Fear of lightning is honored with a long name—astraphobia—and is ascribed to the same cause. Flushing and fidgitiness, we are also told, are symptoms of nervous exhaustion. Indeed, after reading through the long list of symptoms, we came to the conclusion that the whole world must suffer from neurasthenia.

But seriously speaking, we think that the author's position in regard to the symptoms which belong to nervous exhaustion, can hardly be sustained. Under the head of morbid fears he includes symptoms which are generally considered as evidences of mental disorder. A patient who suffers from "agoraphobia, or fear of places" to such an extent that when riding on a Broadway stage he suddenly "shrieks with terror," (page 30), is certainly more than neurasthenic. Still the author declares that these conditions are not found in insanity itself, and that "the habit of calling

them forms of mania or delusions is not based on a right study of these cases." If this is true, then our best authorities are wrong.

We find a distinction insisted upon between hysteria and neurasthenia, but the two conditions are often confounded. Moreover, in making the differential diagnosis between the diseases, the author describes only the mental form of hysteria.

The writer is sanguine to the prognosis. He believes that "the majority of cases can be relieved or permanently cured." He fails to consider, however, the great difficulties surrounding such cases, and the impossibility in many instances of placing the patient so that treatment can be of avail.

We looked with interest to the chapter on treatment, but were disappointed in finding anything new. The general principles laid down are as follows :

1. "The treatment should be constitutional, with special attention to local manifestations whenever they become severe. 2. Dependence should not be placed on any one exclusive mode of treatment, but rather on a combination of various methods, local and general. 3. The treatment should be occasionally changed." It is noticeable that in the treatment advised are to be found the general elements of the plan devised for such cases, and fully described by Dr. Weir Mitchell in his little book entitled "Fat and Blood." We are surprised, therefore, to find only the most scanty recognition of Dr. Mitchell in connection with this treatment. It is generally admitted that he was the first to employ this method for the relief of cases of neurasthenia, and yet the only acknowledgment the author makes of this is to remark, when recommending rest and isolation that "the observations of Mitchell and Goodell on this point are verifiable."

Dr. Beard does not make himself altogether clear as to how far he believes in the rest treatment, for although he advises it, and says that "many years ago he observed that nervous patients were better on Sundays when they did nothing, than on other days. * * * There are patients who need to make every day a sabbath, to have sixty or ninety, or more consecutive days of rest." On page 143, however, he remarks that "nervous invalids do not always need to suspend all labor of the brain, better indeed that they should be actively though pleasantly employed."

The author's dependence in treatment seems to be electricity, rest and massage with the use of certain drugs. The chief medicines employed are the bromides, caffein and strychnia. "Fats and oils, as fish, oysters (*sic*), butter and milk," are regarded as useful adjuncts.

Counter irritation in the form of blisters, and the actual cautery are strongly advised in certain conditions.

Some interesting remarks are made on metal-therapeutics and metalloscopy, but the author does not seem to have used this plan of treatment in any case of neurasthenia. The same may be said of Lomi Lomi, a kind of massage used by the Sandwich Islanders, which is described at length.

The use of cathartics in this class of diseases is a new suggestion, and we believe is often a good plan of treatment. We can not think, however, that the benefit which follows purgation is from its action by counter irritation (page 168). Although electricity is frequently and strongly urged in the treatment, no definite directions are given as to the best manner of applying it. The writer seems to prefer general faradization, and says "as I now use this method in many cases, but slight undressing of the patient is needed." [W. S.]

Neuralgia: its Nature and Treatment. By THOMAS STRETCH DOWSE, M. D., etc. New York, G. P. Putnam's Sons, 1880, pp. 198.

This, the second volume of a series on diseases of the nervous system, is in many respects inferior to the first volume. Instead of being written as a somewhat systematic treatise, with the object of enabling the student or practitioner to form a scientific conception of neuralgia and of its rational treatment, the book is an accumulation of cases with the author's varied experiments in their treatment—strange to say, almost always successful. Many of the cases given are imperfect in respect to pathology, and in very few of them is the indication for treatment specified. In short, the work is a record of empiricism, and as such will please that unfortunately large class in the profession who clamor for "practical works," *i. e.*, books with plenty of treatment. Yet some important modes of treatment of neuralgia receive but a passing notice, not at all commensurate with the progress of the last five years—I refer to galvanism, nerve-stretching, aconitia phosphorus and deep injections. The author's doses are often extra liberal, and we would not advise any one to try the treatment on p. 32, where it is stated that a deep injection of $\frac{1}{4}$ grain morphia, and atropine $\frac{1}{20}$ grain was given; such a dose of atropia would make a patient most uncomfortable and probably delirious. On p. 43, a combination of gr. $\frac{1}{2}$ opium and gr. $\frac{1}{2}$ calomel is recommended to be given every two hours to bring the system under

mercurial influence; doses which would develop opium symptoms much sooner than ptyalism in our opinion. On the other hand, ten grains of quinia is spoken of as a "large dose," (p. 59.)

At the end of the volume, under the head of special neuralgias, are grouped miscellaneous cases of pain whose neuralgic nature is doubtful, etc., etc.

There are many things about the volume which, besides its unscientific construction, would lead one to believe that it was written for popular sale. Its language is studiously adapted for the lay understanding. On p. 47 a tumor is described as "on the outermost covering of the anterior lobe of the brain;" syphilis is not named and fully discussed as it should be on p. 89, and not named on p. 43, where the author claims the efficacy of mercury and iodide of potassium. Again, on p. 69, "neuralgia between the ribs" is the heading of a section. On pp. 70–71 Dr. Dowse tells a patient's friend that she "had diffuse tubercle which was lying latent, but which would be the death of her."

The author's wonderful success in the treatment of all neuralgias is also worth noting, as well as his alarmist statement on p. 57: "If a neuralgia does not yield to treatment within a given time—and I am referring now to neuralgias of advanced life—the patient generally dies from exhaustion, or from apoplexy, or some form of insanity supervenes, from which recovery never takes place." [E. C. S.]

ABSTRACTS AND SUMMARIES.

Case of Labor with Avulsion of the Uterus; Recovery.

Dr. Nieprasch of Cüstrin, relates the following extraordinary case: A woman, aged twenty-nine years, was delivered for the seventh time with the assistance of a midwife, who, to expedite a foot presentation, exerted violent traction upon the child, and later upon what seemed to be an attachment of some sort to its head. Excessive hemorrhage and collapse ensued, and Dr. Nieprasch was called. He arrived about two hours after the violence had been done, and found the patient blanched, pulseless, collapsed; lying in a great pool of blood which had run through the bedding and reached the floor. Between the patient's thighs lay a dead child with its cranium still encompassed by the uterus, which also lay entirely outside of the external genitals, connected with the body only by one round ligament. A small portion of small intestine was also lying without the vulva. The physician contented himself with carefully extricating the head of the dead fœtus from the uterus (!) and wrapping the displaced organs in a linen cloth well oiled. He thought the mother was as good as dead, did nothing for her (!) and told the husband to call him in the morning if she were still alive.

The next day, with a consultant, Dr. Gerlach, the patient was visited, the round ligament tied and cut, the displaced small intestine carefully replaced in the abdominal cavity above the distended bladder, a large sponge soaked in a 2 per cent. solution of carbolic acid was placed high up in the vagina, and the bladder emptied by a catheter. An antiphlogistic diet and a little red wine were ordered. On the day after the operation, the patient's temperature rose to 40° C. without chill, and the removal of the

sponge gave issue to considerable ichor. The sponge dressing was daily renewed. On the fourth, and frequently in four succeeding days, mucous stools were passed; the pulse sank to, 140; some appetite appeared. Catheter used daily by patient's husband. A slight vaginal discharge continued. On the fourteenth day the bowels were well moved by 15 grm. of castor oil and an enema. On the sixteenth day the first laudable pus came from the vagina. The local treatment consisted in the insertion, three times a day, of a small carbolized sponge. Fever ceased on the twenty-third day, and a digital examination showed a well-vaulted vaginal cavity about 8 cm. long.

This case is a wonderful illustration of the resistance of the human organism to injuries, and of the strong tendency to recovery and repair in spite of adverse circumstances. The patient was, most practitioners would think, inhumanly neglected during the first twelve hours following the injury; she lived in a hovel (seven persons in two small rooms), she had to lie in a filthy bedding for several days, and the husband was cook and nurse, having even to draw off the urine. It is probable that the carbolic acid dressing, feeble though it was (2 per cent.), saved this life.—(*Berliner Klinische Wochenschrift*, No. 27, 1880).

[E. C. S.]

Narcolepsy.—Under this singular, though perhaps appropriate name, Dr. Gélineau, of Paris, describes a remarkable uncomplicated case of sleeping sickness.

During two years the patient, a man aged 38 years, suffered from frequent (as many as two hundred a day) attacks of irresistible sleep, with drooping of the lids, a sense of helplessness in the extremities, staggering, and often falling. Even in the worst seizures consciousness was preserved, and at no time were any convulsive movements observed. The eyelids were always closed, and the pupils never largely dilated. The exciting causes of seizure were various in kind, usually of a psychical order; an emotion, yet always a pleasurable one, such as anticipating a pleasure, or foreseeing a successful bargain, or meeting a friend, was quite certain to make him sleep for one or five minutes. During the sleep the pulse, which was normally 66–68, fell to 58–60 per minute.

The author insists that the symptoms as given above justify a diagnosis from petit-mal, in which the eyes are open, the pupils wide, face pale, and patient unconscious, though but for a mo-

ment. [He might have added the duration of the seizures, for petit-mal seldom lasts a minute.] Bromide of potassium and various modes of treatment remained without effect.—(*Gazette des Hôiptaux*, No. 79, 1880). [E. C. S.]

On the Pathology of Night-Sweating in Phthisis, and the Mode of Action of Strychnia and other Remedies in it.

Lauder Brunton believes that the pathology of night-sweating in phthisis, to be as follows: The respiratory centre becomes exhausted by the reflex irritation from the lung, so that it no longer responds so readily as it ought to the stimulus directly applied to it by carbonic acid in the blood circulating through the medulla and through the spinal cord. In consequence of this the blood becomes more or less venous, and to this venosity, and the consequent imperfect tissue change, and not as was formerly supposed, to the actual loss of fluid or sweat in the sleep, are the nervous and muscular exhaustion and prostration observed in night-sweats to be attributed. If this pathology were correct, it occurred to him that night-sweating might be prevented by administering some remedy which would increase the excitability of the respiratory centre. Now, such a remedy exists in strychnia, as has been shown by Rokitansky's experiment. If then, a dose of strychnia or nux vomica were administered at bedtime, the excitability of the respiratory centre ought to be so much stimulated that any excess of carbonic acid would excite it to increased action, and thus the accumulation of carbonic acid in the blood would be prevented, and the sweat, which he had supposed to be the consequence of such accumulation, would be arrested.

On proceeding to try it in the wards of St. Bartholomew's Hospital, this expectation was realized.

But it is evident that if strychnia increases the excitability of the respiratory centre by stimulus applied directly to it, such as the carbonic acid present in the blood, it is likely also to increase its susceptibility to reflex irritation, such as that excited by tubercle in the lungs, and thus it might be that it would tend to increase the cough in case of phthisis. This, indeed, it appears in some instances to do. He has tried to remedy this by a combination of strychnia with opium, and this appears partially to succeed. Where, however, strychnia does not appear to suit, atropia may answer perfectly.

In Dover's powder we have a combination having an action

somewhat resembling that of atropia in certain respects, though differing from it very markedly in others. In health, Dover's powder is a powerful sudorific, but it frequently arrests, in the most satisfactory manner, the night-sweating of phthisis.

This seems at first sight to be extraordinary, and yet it is quite natural if the view which he has advanced regarding the pathology of night-sweating in phthisis be correct. For the opium, by lessening the irritation by cough, will tend to prevent the exhaustion of the respiratory centre. At the same time ipecacuanha is a powerful stimulant to this centre, and thus we have in Dover's powder two of the actions that we have already observed in atropia, viz., a power of diminishing irritation from the lungs, with a power of increasing the activity of the respiratory centre. Unlike atropia, it does not paralyze the peripheral terminations of the secretory nerves in the sweat glands. Picrotoxine, also, has been found to be useful in night-sweating. It also is a powerful stimulant to the respiratory centre (Büchheim, *Arzneimittellehre*, *3te Aufl.*), and probably it is by its stimulating action upon this centre that it arrests sweating. But while it is probable that the night-sweats of phthisis chiefly depend upon exhaustion of the respiratory centre, and are to be arrested by stimulation of this centre, we must bear in mind that this may not be the only cause of such sweats. They may occur through stimulation of the sweating centres by increased temperature as well as by increased amount of carbonic acid in the circulating blood. In such circumstances quinine will probably be the best remedy.

In the night-sweats of phthisis atropia is probably the most powerful remedy which we possess, and we can well see how it should be so, for it combines the power of lessening irritability of the sensory nerves in the lung, of stimulating the respiratory centre, and of paralyzing the ends of the secreting nerves in the sweat glands. But it possesses other actions which may render its employment inadvisable. It may so influence the salivary glands as to arrest their secretion, and cause very great discomfort to the patient by the dryness of the mouth thus occasioned. In such cases we may use Dover's powers, but if this, again, should interfere with digestion, we may resort to strychnia or nux vomica. (*St. Bartholomew's Hospital Reports*, Vol. xv, 1877.) [McB.]

Acute pernicious (?) anæmia; Recovery. Dr. E. Hansen reports, in a very complete way, a case of progressive anæmia in which recovery ensued under a simple treatment by lattate of

iron (.10 grm. four and six times a day) and diet. Shortly after the admission to hospital an enumeration of the blood showed 1,100,000 globules per cmm.; Sept. 21st the number had diminished to 425,000 globules (!); after this period a wholly unexpected improvement took place, the number of globules rising rapidly: Oct. 22d, 1,600,000; Nov. 2d, 2,400,000; Dec. 13th, 3,700,000; March 15 (1879), 5,000,000. Several retinal hemorrhages occurred from time to time, but underwent complete resorption. The paper contains tables of urinary analysis, and of frequent blood-countings. At no time did the proportion of white blood globules suggest leucocythæmia. The red globules were usually of normal diameter, 5, 7 and 10 μ.; but during convalesence a large number of small (young?) globules, measuring 3 to 4 μ. appeared. (*Nordiskt Medicinskt Arkiv*, Bd. xii H. 1, p. 1). [E. C. S.]

Physiological effects of Friedrichshall Bitterwater. Dr. J. von Mering has made an experimental and clinical study of the action of this well-known water, and has reached the following conclusions:

1. F. water increases the appetite, and has no ill effect on health.

2. It has a diuretic and purgative effect.

3. It exerts a marked influence on the albuminate of the body, as shown by increase of the urea and phosphoric acid formation.

4. In F. water we have combined the different actions of chloride of sodium and sulphate of sodium. (*Berliner Klinische Wochenschrift*, No. 11, 1880.) [E. C. S.]

ORIGINAL OBSERVATIONS.

ON OCCIPITAL HEADACHE AS A SYMPTOM OF URÆMIA.

By E. C. SEGUIN, M. D.

I have recently met with two cases in which occipital headache was so localized and persistent as to give rise to a strong suspicion of organic disease of the cerebellum, and in one of them a positive conclusion was only reached by means of a *post mortem* examination. These cases both now appear to have been cases of contracted kidneys and uræmia.

I shall first relate the cases as they are in my case-book.

Case 1.—Lieut. X., U. S. A., aged 36 years, consulted me on November 5, 1879, and gave the following history; Until the time of his graduation from West Point, he had suffered from frequent general headaches; but that since leaving the school in 1867 he had had several severe attacks of occipital headache. These at first occurred two or three times a year, but in the last few years much more often, the attacks lasting from twenty-four to forty-eight hours, accompanied by vomiting and sometimes by delirium. These paroxysms were often relieved by bromide of potassium. In 1876, during Centennial times, he had one of his headaches, and with it an epileptiform convulsion, in which he did not bite his tongue. In February, 1879, at the same time with a headache, he had another convulsion, in which the tongue was bitten. He has noticed that while in the Northern States he has but few headaches, whereas when in Texas he has had a great many. He often has a feeling of soreness and fulness in the back of the neck, and is very nervous after the attacks; has been in the North since March. In August had a severe attack and another on October 26, aborted by bromide of potassium. This last headache was accompanied by stiffness and fulness in the

back part of the neck. There are no special ocular symptoms during the attacks, and he considers his eyesight normal. During the paroxysms the face is flushed, the head feels full and pulsating. The father and the grandfather of the patient had sick-headache.

Mr. X. has abstained from the use of intoxicating drinks since 1876; he has never been injured about the head, and has never had syphilis.

Examination.—Eyesight normal to all ordinary tests; no astigmatism; no lesion seen with the ophthalmoscope. Cervical spine not tender; no symptoms of dyspepsia; heart normal. The general appearance is that of health. November 6th, looks puffy under the eyes. Three specimens of urine are examined with the following results: Their specific gravity is low, ranging from 1018 to 1020; they all contain albumen—from 1 per cent. to .5 per cent., and hyaline casts. The retinæ are reëxamined with negative results. Subsequently, numerous examinations of the urine were made by Dr. Alexander, Surgeon U. S. A. at West Point, and evidences of chronic Bright's disease were invariably found, such as low specific gravity, hyaline and granular casts; the amount of urea in one period of twenty-four hours was about twelve grammes.

Case 2.—Mr. J. W., a merchant, aged 47 years, was seen by me at Passaic, N. J., in consultation with Dr. J. C. Herrick, on December 21, 1879. I obtained the following hisotry of the case:

The patient had formerly enjoyed good health; had never received any injury to the head; no syphilis. During all his adult life he has suffered from headaches, more or less periodical, perhaps one in three weeks, each attack accompanied by nausea, and usually lasting one day. Of late years he has had much less of this headache; it was evidently migraine. About twelve years ago, in the streets of New York, during hot weather, he had an attack which was called "sunstroke." The symptoms of this attack are unknown. Mr. W. consulted me in 1874, but I have no notes of his case except a memorandum of my examination of the urine. This appears to have been perfectly normal.

In the last two years he has been almost constantly suffering from some headache, a little every morning, and more and more often of late he has had severe attacks. In the past two months very severe headaches, with nausea and vomiting several times a week. During the last two weeks has been confined to his bed. The patient and his wife clearly distinguish this pain from the

former headaches by several characteristics; the pain is more violent, it is distinctly occipital, and lately has been cervical as well; it appears in paroxysms at any time, chiefly during the day, and the pain itself is of a different character. After a migraine Mr. W. felt very well; but now after a severe headache he is prostrated and dull. The nausea always comes on after the pain; he has no nausea between the paroxysms. He has not had much frontal headache, but the pain has extended from the occiput into the vertex and the whole top of the head. Movements aggravate the pain. There is no affection of sight or hearing; no dizziness. Of late has needed morphia; Paullinia seemed effectual for a few days only. During the last week he has taken about 4 gm. of bromide of potassium a day, and on the day before yesterday he had 15 gm. in twenty-four hours. The attending physician has examined one specimen of urine, but found no albumen.

Examination.—Patient feeble; lies relaxed in bed; voice faint, but articulation is distinct; mind clear; the head is not tender. The right eyelid is in partial ptosis; no strabismus; the ophthalmoscope shows no lesion of the fundus (atropine used). The right side of the face is rather inert, but the tongue (heavily coated) points straight. The hands are of due proportionate strength; in walking the right foot is dragged after a few turns in the room. No incontinence of urine; morphia affects patient very readily; he has had none in twenty-three hours, yet he is dull, and his pupils are small and fixed. The heart is normal, but the pulse is quite irregular; beating 23 and 29 in successive thirds of a minute; twice in the minute an acceleration is noticed. There is a trace of œdema on the tibias. Patient denies that his neck is really stiff, though he carries his head on one side, and keeps it quite still; no opisthotonus. To-day the pain extends to the sixth cervical vertebra.

I declined to give a positive diagnosis until after the urine had been thoroughly examined. At the same time I saw that the patient was in great danger from exhaustion and a tendency to stupor; and that many of the symptoms of tumor in the cerebellum were present. One was lacking, viz: neuro-retinitis. I also thought he was brominized.

On December 23, three specimens of urine were received, and were at once examined by Dr. R. W. Amidon. The specific gravity was found to vary between 1024 and 1025; there was albumen in all, varying in amount from 3 to 10 per cent.; there were also in all specimens numerous hyaline and granular casts.

Mr. W. died on December 27th, in a comatose state ; no convulsions or further paralytic symptoms having shown themselves. The autopsy made on the 28th, showed that the cerebellum and the other parts of the encephalic mass were normal ; while both kidneys were extensively diseased. The left kidney was found completely diseased, granular and hard in places ; its membranes peeling off with difficulty. It had a reddened congested appearance, and showed some evidences of not only a chronic trouble, but of a more recent acute inflammatory action. The right kidney was found to be only partially affected ; somewhat congested, and with the same type of lesion.

Dr. Herrick, to whose courtesy I am indebted for the above account of the autopsy, adds : " The results of our examination go to show evidently that, after all, the patient's symptoms may have originated from a kidney disease, although we cannot explain yet why his headaches so many years should have been from such a cause. He had never complained of backache or of any of the usual symptoms of Bright's disease, except the head pain."

The following is a summary of the symptomatology of the two cases :

Both patients were adults ; both had suffered from chronic headache more or less of the migraine type ; at a given period the headache became transformed into a localized occipital pain, very different from that of the former attacks.

In Case 2, the pain extended down the cervical spine, and was so much aggravated by movement as to suggest a rigid state of the neck. In Case 1, there was once stiffness of the neck in an attack.

This peculiar headache was distinctly paroxysmal, but not at all periodical or influenced by any apparent outward circumstance. In both cases nausea accompanied the headaches, and in Case 2 it is clearly stated that the nausea was secondary in point of time.

Case 1 was made relatively clearer by the previous history of convulsions, and by the fact (not stated in the notes, but quite clear in my recollection) that the surgeons in attendance then (in 1876) found albumen in the urine.

Case 2 was greatly complicated by the presence of symptoms of slight paralysis, partial ptosis and a weak right leg. I am now disposed to think that these phenomena, together with the astonishing debility, staggering gait, and the sluggish state of the mind which I observed in this patient, were due

to brominism : a condition to which I have called attention as a possible serious complication in the diagnosis of disease.*

I would also remark that the symptoms of renal disease were not marked ; in one case there was no œdema, in the other a mere trace ; neither patient had the dyspeptic symptoms or the frontal headache which often suggest renal disease, and neither patient has the "Brighty look" which is so well known.

It is to be observed that the occipital sensation in these cases was true pain, not the painful paræsthesiæ which are sometimes due to lithæmia and oxaluria, and sometimes to eye-strain, and which are erroneously (or rather insufficiently) designated as cerebral hyperæmia.

In some respects the story of these cases is imperfect, and I particularly regret the lack of observations upon the quantity of urine passed, and upon the state of the arterial tension.

Still I am inclined to believe that the publication of these cases may serve to render more accurate the diagnosis of occipital headache, and to illustrate the utility of critically examining the urine in cases of any degree of obscurity ; more especially as occipital headache is scarcely mentioned as a symptom of uræmia.

A CASE OF MYSOPHOBIA.

By E. C. SEGUIN, M.D.

Miss X., aged 18 years, consulted me on January 30, 1880, for a peculiar form of nervousness.

From the patient's mother I obtained the following history of the case. Childhood and girlhood had been healthy ; menstruation began in the twelfth year, and has since been normal and regular. Patient has led an idle, luxurious life, doing as she pleased, and taking a few private lessons at home. Has been allowed to rise at 8 or 9 A.M., to lounge about and read trashy novels. Was of a bright, happy temperament. The occurrence of insanity in the family is denied, though maternal grandmother had senile dementia, and one brother had fits at six months followed by hemiplegia, epilepsy and imbecility. As bearing on the case it must be mentioned that patient's grand-uncle died of "cancer of the nose."

About three years ago patient had a moderate leucorrhœa, re-

* The Abuse and Use of Bromides.—*Journal of Nervous and Mental Disease.* July, 1877.

lieved by a tonic course. After this, was observed to droop and look tired, and she began to entertain the hypochrondriacal fear or delusion that she too had or was to have cancer of the nose. (It was not till last summer that she confessed this notion, which was the cause of her inexplicable melancholia.) Ever since, Miss X. has been the victim of extreme hypochondriasis and mysophobia. The hypochondriasis consisted exclusively in fear of internal nasal cancer, and many of her first peculiarities as to washing herself and fear of contamination were logically related to this idea ; she fussed with her hair, face and hands, in order to prevent others from catching the cancer which she had.

It is useless to relate all the details of the morbid cleanliness of the patient. She would wash her hands every five minutes, and wipe each finger carefully and long. She would spend an hour or more in the bath-room, and use a dozen or more towels for her morning toilet. She would spend an hour or more in combing and brushing her hair. Great slowness in all acts is observed. Avoids touching door-knobs, plates, chairs, etc., without protecting her hand by a glove, or a fold of her dress ; refuses to pass dishes, etc., to others at table ; will re-arrange a chair several times ; has been seen to approach a door and retreat several times before passing through it ; has stood in the middle of a room with an absurd automatic pendulum movement of whole body or of one arm ; is reluctant to wear clothing, especially under-clothing, more than a day at a time.

Has bad, restless nights ; at times is rather excited, and will pace up and down a room ; usually inert, lies on a lounge with a novel ; has seemed as intelligent as ever ; at times pain in head and in mid-dorsal region ; no hysteria or agoraphobia, or delusions of any other sort than the above.

The examination is nearly negative. Miss X. presents the appearance of health and is rational. She admits the absurdity of her freaks and notions, blushes and attempts to hush her mother in the relation of details. Even as regards the original hypochondriacal notion of cancer in the nose she is not firm ; having nearly lost her belief in that disease since a thorough examination of the nose and throat was made by a specialist some months ago. Explains her actions by her desire that no one should acquire the cancer from her. Hands are chapped by constant wetting ; spine not tender ; tongue clean ; heart normal, and pulse good. Has a semi-melancholic rather dull look, and eyes easily fill with tears. Denies masturbation.

I carefully laid out a plan of moral treatment by which she was to be gradually and firmly prevented from doing her strange acts. She was ordered long walks and a little study. Everyone near her was instructed to assume a semi-imperative tone and manner toward her. She was to clean her own room.

I decided to try a course of treatment by narcotics, similar to that which succeeds so well in mild melancholia. She was given extracts of opium and cannabus indica with a little rheubarb. Feb. 24th, great improvement is reported ; patient has gained self-control and is cheerful ; sleeps well. On occount of nausea, the opium was omitted and a pill of cannabus indica, reduced iron and rheubarb ordered. At bed-time 1.5 gm. of KBr. March 15 ; has been in the country two weeks, taking above remedies and walking a great deal. Is well ; advice, discontinue medicine, but occupations to be increased, exercise to be continued, and a firm moral hold to be kept on the patient.

I shall add that Miss X. was not allowed to know the nature of her medicines.

While recording the case of this singular form of hypochondriasis, I desire to express my belief that a relapse or the development of a different and more serious psychosis in the patient is very probable.

Mysophobia was first described and discussed by Dr. William A. Hammond, in Neurological Contributions, Vol. 1, No. 1, p. 40. N. Y., 1879.

ARCHIVES OF MEDICINE.

Original Articles.

SPINAL MYOSIS AND REFLEX PUPILLARY IMMOBILITY.*

By Dr. WILHELM ERB,

PROFESSOR IN THE UNIVERSITY OF LEIPZIG.

AMONG the symptoms recently added to the description of tabes dorsalis, through the researches of neuro-pathologists, and recognized as significant for the same, the phenomena designated as "*spinal myosis*," or better, as "*myosis with reflex pupillary immobility*," will here claim more than ordinary attention.

By the above term is understood that condition of the pupils, in which they usually present a very decided contraction—myosis—and at the same time are completely inexcitable to light, while under accommodative impulses and convergence of the visual axes, they react with perfectly normal promptness.

Discovered and described in the year 1869 by Argyll Robertson,[1] this peculiar condition was confirmed and elaborated in isolated cases by Knapp and Leber,[2] then from somewhat richer material by Wernicke[3] and Hempel.[4] Hempel (Leber) in particular demonstrated its more frequent appearance in tabes dorsalis, but with Vincent[5] orig-

* Translated by Dr. W. R. Birdsall of New York, from the author's manuscript.

inated the first productive observation and elaboration of the subject. He investigated it in tabes, in progressive cerebral paralysis, and in several other diseases of the nervous system, in a large number of cases, and arrived at very important and remarkable results.

I have thoroughly discussed this subject myself in an extensive work on the pathology of tabes dorsalis,[6] from the standpoint of numerous personal observations, and arrived, in the main, at the same results reached by Vincent.

The results, up to the present time, of the observations in question, may be briefly condensed as follows:

The symptom, reflex pupillary immobility, as I have called inexcitability to the stimulus of light with preserved accommodative movements, appears with remarkable frequency in tabes dorsalis, being one of its most constant signs. It is frequent even in the initial stage; in the ataxic stage being in fact constant. It is not always associated with myosis, however, for the pupils may be of normal expansion, or even abnormally dilated. In the latter stages of the affection, myosis is certainly more frequent, in fact, the rule. Even with amblyopics, reflex immobility is frequent, and myosis may be observed, not infrequently, in complete amaurosis. In progressive paralysis of the insane the reflex pupillary immobility is almost as frequent as in tabes, but associated less frequently with myosis; far more often with inequality of the medium-sized pupil. Under all other circumstances the appearance of this phenomenon is very rare; yet it is to be observed in many other spinal and cerebral affections, in syphilis, etc. For the more exact semeiotic valuation of this phenomenon, however, there is lacking the positive determination of *how early* it appears among the symptoms of tabes; where, under what circumstances, in what other diseases it is present; and what particular accidental circumstances, perchance, accompany it, etc. Fur-

ther statistical investigations concerning its frequency, particularly in tabes, would also be desirable.

Since my last publication, above mentioned, I have followed this subject uninterruptedly, and now present again eighty-four new observations of typical tabes, collected since that time at all stages of the disease, mostly, however, from the earlier stages, in which the symptom was more carefully considered; besides, I have followed the same, as far as possible, in other diseases, and in healthy persons.

The best method of testing this phenomenon I have given in my essay cited above (p. 32). I only repeat here, to emphasize, that confusion of accidental accommodative movements with contractions of the pupils, has to be guarded against in particular. It is, therefore, not advisable to test the reaction to light by opening and closing the lids, but it is preferable to inspect the pupils attentively while the eyes are alternately shaded and illuminated. With the same caution it is also expedient to use, by means of a convex lens, natural or artificial light, for the purpose of examination, by which, oftentimes, traces of reaction are discovered which were not recognizable by ordinary illumination.

The results of my observations are as follows:

In 84 cases of tabes dorsalis, absolute reflex immobility occurred 59 times; very weak, slow, and inexpansive reaction to light, 12 times; hence, a total of 71 cases with diminished reaction, against 13 with normal reaction. Expressed according to percentage, a more or less high degree of reflex immobility of the pupils was present in 84.5 per cent. of my cases of tabes, and absent in only 15.5 per cent. Out of these 71 cases there were 3 in which the reflex immobility was unilateral, and 7 in which more or less marked optic nerve atrophy existed. Only 37—that is, 52 per cent.—were associated with decided myosis; and of normal-sized, with,

at the same time, somewhat unequal pupils there were 34 cases. Myosis, therefore, is not, at all events, regularly associated with reflex immobility, but fails in fully one-half the cases. Among the 37 cases with myosis were 8 with optic nerve atrophy. Out of the 71 cases, 43 belonged to the initial stage—that is, they exhibited no signs of ataxia, or only the slightest traces of it. The remaining 28 cases belonged to the ataxic stage—meaning, that they came under observation for the first time after having presented the phenomena of ataxia for a longer or shorter period. From the latter group of cases certainly no conclusions can be reached respecting the commencement of the disease, and the former are not much more conclusive, for the initial stage often lasts extraordinarily long—twenty-five years or more. At the same time, this symptom *may* appear very early in the course of the disease. In a complete series of cases I have seen it after a duration of the disease of from two to three years, and in one case each, after one and a-half years, one year, eight months, and six months. On the contrary, I have also seen it absent after the disease had continued for five, six, and ten years; in one case even after nineteen years! It does not, therefore, *necessarily* appear, but may be completely absent during the whole course of the disease. Yet, notwithstanding this, it belongs manifestly to the phenomena which are presented with the greatest regularity in tabes, and appear in the majority of cases very early.

It seemed to me of particular interest to test in what relation the reflex pupillary immobility possibly stood to that which is so important in the etiology of tabes, viz., syphilis; all the more, since in several cases of syphilitic infection I had seen that symptom appear alone, and as the percentage of cases of tabes presenting reflex immobility in the course of the symptoms, and of those presenting

syphilis in their previous history,' were nearly the same, the thought occurred that perhaps the reflex immobility and the myosis might appear exclusively, or predominantly, in those patients with tabes who had formerly had syphilis. The facts teach that this idea is not correct, for, from a review of my case-book, I find nine cases of tabes without previous syphilis, in which, at the same time, myosis and reflex immobility were present; and on the other hand, ten cases in which these phenomena were absent, although syphilis had preceded the development of tabes. A closer causal connection, therefore, between syphilis and reflex pupillary immobility does not appear to exist.

Concerning the occurrence of pupillary phenomena in progressive cerebral paralysis, I am not in a position to furnish much material, and even that little which I have observed myself, can claim no great worth, as, naturally, only those cases presenting few mental disturbances at the very beginning of the disease come to me. I have notes on 16 such cases, among which appeared all possible pupillary changes; in 2 cases only, nothing abnormal was to be found. In 10 cases, on the contrary, inequality of the pupils existed; but of these there were 7 in which the reaction to light was not appreciably diminished, and only 3 in which equilateral reflex pupillary immobility existed. Finally, myosis, equilateral and with reflex immobility, was present in 4 cases.

These few observations, which, as already mentioned, relate to the early stages of progressive paralysis, confirm, therefore, not only the great frequency of pupillary changes in this disease, but also the predominance of pupillary inequality over the remaining changes, particularly over myosis, less over reflex immobility. Inequality of the pupils I found 10 times in the 16 cases; on the other hand, reflex immobility, 7 times—once on one side only. There

is nothing further to conclude, however, from this small number.

In consequence of the great interest which the symptom possesses relative to the early diagnosis of tabes, I have naturally directed my attention to the presence of this disturbance in all manner of persons, and particularly to patients with diseases of all kinds. I have recently been able to demonstrate the existence of exquisite myosis, with reflex immobility, in patients not affected with tabes or progressive paralysis. A few of these cases may be reported here:

1. Male, æt. 45, born healthy, with anæmia in the highest degree (pernicious?), without any symptoms of tabes or other neurotic appearance, exhibited a very marked bilateral myosis with reflex immobility, together with distinct accommodative mobility. Under atropine the pupils dilated slowly and moderately.

2. Male, æt. 55, affected for many years with tremor, and all sorts of vague, neurotic disturbances—perhaps from chronic nicotin intoxication—had myosis with reflex immobility and retained accommodative mobility.

3. Male, æt. 33, twelve years previous had syphilis, has now mydriasis and paresis of accommodation of the right eye; in the left eye decided myosis, and bilateral reflex pupillary immobility. Except unusual fatigue of the legs has no trace of other tabetic symptoms.

That these patients will not develop tabes later, who will dare to say?

I have seen, besides, though rarely, this symptom in one or both eyes, in several other cases of neurotic disease, belonging mostly to central affections difficult of determination, but which could neither be classed with tabes nor progressive paralysis.

It is known, further, that in diseases or injuries of the cervical sympathetic, myosis appears, but, at the same time, the reaction to light is apt to be retained. The same holds true for lesions in the cervical region of the spinal cord.

The question is very important, finally, whether myosis with reflex immobility appears in healthy individuals. I have not, as yet, seen it. The presence of myosis is certainly very common in aged people, and even in a great number of young persons, who present absolutely nothing pathological; but I have never seen reflex pupillary immobility associated with it, and can confirm the assertion of Hempel, that in uncomplicated senile myosis the reflex reaction of the pupils to light is preserved.

The facts teach, therefore, that the symptom, reflex pupillary immobility, with or without myosis, is excessively frequent in tabes dorsalis and progressive paralysis; appears very rarely in other neurotic and in all other diseases; and apparently not at all in healthy individuals.

In order to understand correctly the value of this symptom and its signification in tabes, it is necessary to investigate its pathologico-anatomical and pathologico-psychological foundation somewhat more closely; for, evidently, this symptom, which appears so early and so regularly, is of the greatest interest for the understanding of the early localization of tabes so frequently in the region of the cranial nerves and the superior part of the cervical cord.

Entering upon the nature of the manifestation more closely, we are taught at once that we have here—and this point has also been developed by former observers—two things to be separated, differing from one another according to their nature and localization, namely: myosis, and reflex pupillary immobility.

Simple and daily demonstrable facts show that these two phenomena are by no means invariably associated; that reflex immobility can exist, now with, now without myosis; that it may appear in normally, or even in abnormally dilated pupils; and that, on the other hand, myosis very often occurs without reflex immobility—for example in healthy

persons, and with lesions of the sympathetic—showing that we have to do with two different sorts of phenomena, which are to be referred to different pathological factors. Indeed, it is easily possible, according to principles of current views concerning the innervation of the iris, to represent, schematically, a picture of when, and how the two disturbances may occur.

Between the retina and the sphincter pupillæ, innervated by the oculomotorius, exists a reflex tract, which goes through the optic nerve to the brain, within the brain to the oculomotorius, and in this, centrifugally to the iris. The reflex connection between the optic and oculomotor nerves within the brain takes place, probably, through the central apparatus intercalated in these nerve tracts. In explanation of the reflex immobility we can only maintain that that portion of the reflex tract is affected which lies between the optic and oculomotor centres just described, for, if the lesion were located in the peripheral optic tract, then the vision could not remain normal; while, if the peripheral oculomotor tract were the seat of the lesion, the pupils could not contract from accommodative impulses. We must, therefore, consider that small portion of the reflex tract—the location of which needs in many respects to be more exactly determined—as the diseased part whence originates the reflex immobility.

For the myosis we must refer to changes in the dilatator pupillæ, whose centre is said to be in the medulla oblongata, and sends the principal part of its mass in the cervical cord down to the cilio-spinal centre. A spasm of the sphincter cannot well be conceived with retained accommodative movements, and with a duration of the condition for many years. The tone of the dilatator pupillæ is controlled and regulated from those centres, it appears, partly by reflex excitations conveyed to them through the posterior roots

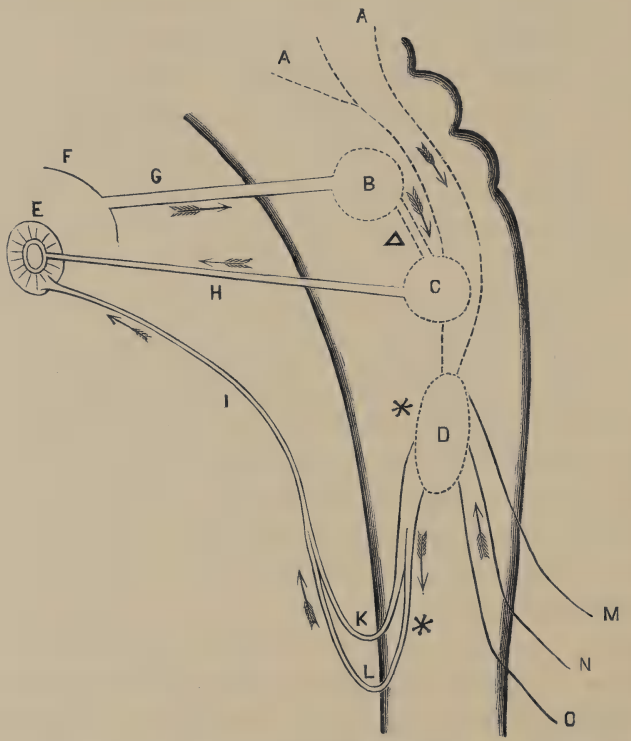
from the periphery of the body, and partly from the brain, through psychical and other excitations. The centrifugal tracts which extend from this centre to the dilatator pupillæ lie, as is well known, principally in the cervical sympathetic. It is scarcely to be maintained that the myosis of tabes should result through the removal of *all* these centripetal—peripheral and central—excitations. The regular absence of anæsthesia extending over the *whole* cutaneous surface speaks at once against this view, for the theory of a partial paralysis of peripheral centripetal tracts, conducting centripetal iris reflexes, is hardly tenable; and a corresponding inhibition of all psychical excitations is also inacceptable. In any case, the theory is much more simple that the centre for dilatation of the pupils itself, or the motor conduction-tracts proceeding from it within the spinal cord or in the cervical sympathetic, are either paralyzed or otherwise injured, and that the myosis results in consequence.*

A peripheral affection of the dilator muscle itself is not to be thought of. For the explanation of myosis with reflex immobility in tabes and other diseases, we must conclude that two separately localized lesions are present, which may be sought for from within the cervical cord up to the corpora quadrigemina.

The following little sketch will best illustrate how, and where, the supposed lesions may be situated in the encephalon and cervical cord. The reflex tract is seen to pass from the retina, through the optic nerve to the centrum optici, from this to the centrum oculomotorii, and through the oculomotor trunk to the sphincter pupillæ. The small triangle (△) indicates that point of the reflex tract where the lesion which leads to reflex pupillary immobility is to be

* G. Bessau (or Besau ?) who, under Grünhagen's direction published an experimental essay on "pupillary contraction in sleep and diseases of the spinal cord" (Dissert. Königsberg, 1879), which I have been unable to obtain in the original, states that this view cannot be correct. Why his experiments forced him to this conclusion is not made clear to me from the report in the *Centralblat für Augenheilk.*, Dec., 1879; June, 1880.

looked for. Elsewhere the centrum dilatatorium (ciliospinal) is seen, which conducts centripetal excitations in part from the brain (psychical influences), in part through the posterior roots from the periphery of the body (sensory impressions), and whose centrifugal tracts (for dilating the pupils) leave the spinal cord through the anterior roots, in



AA. psychical impressions; *B.* centrum optici; *C.* centrum oculomotorii; *D.* centrum dilatatorium; *E.* iris; *F.* retina; *G.* optic nerve; *H.* oculomotor (sphincter); *I.* sympathetic (dilator); *KL.* anterior roots; *MNO.* posterior roots; △ seat of lesion causing reflex pupillary immobility; * probable seat of lesion causing myosis.

order to reach the dilatator pupillæ through the sympathetic going to the iris. The asterisks indicate the places where lesions might produce myosis.

In order to further test the correctness or incorrectness of the theory, I have arranged, with the aid of my assistant

at the Polyklinik here, Dr. Kast, a series of experiments on my patients and a number of healthy persons, based upon various well-known facts, the results of which, being possibly of importance, I will briefly report.

The action of atropine was first tested on a number of tabetic patients with myosis and reflex immobility, and also on the two patients mentioned above, with the same affection. With all, dilatation of the pupils took place after instillation of atropine, though slowly and not quite as decidedly as in healthy persons. The same thing has been observed by Argyll Robertson, Leber, Hempel, Vincent, and others. According to Vincent, eserine produces a still greater contraction of the myotic pupil.

It is a recent though frequently studied fact[8] that in a condition of normal sleep a high degree of physiological myosis exists, but at the moment of waking a considerable dilatation of the pupil follows, after which it returns to the medium size. The same experiment was made upon a tabetic patient, with myosis and reflex immobility, in my wards in Heidelberg. During sleep the patient's eyelids were gently drawn apart, and he was awakened by calling, and simultaneously pinching the skin—a double excitation; his pupils remained absolutely immovable and highly contracted. It is known, in addition, that through lively irritation of the skin in a waking condition, as well as during normal sleep, and also in sleep from chloroform, a dilatation of the pupils will be brought about. Based on this fact, I began a series of experiments, the results of which, up to the present time, I will communicate briefly.

If to a healthy person—his face being illuminated, and his eyes quietly but persistently fixed on a more or less shaded object—a strong cutaneous irritation be applied to the skin of the back of the neck, the mastoid process, the breast, the cheek, or the back of the hand, by means of

the faradic brush, for example; the other—moist—electrode resting on the sternum, there follows after a short but distinct interval a more or less considerable slow dilatation of the pupils, which remains for a short time after the removal of the excitant, then slowly returns to its previous condition. The same thing occurs when, instead of using electricity, we sharply pinch a fold of the skin. By repeating the irritation several times in rapid succession, the action becomes visibly weaker, and finally ceases entirely; after a short rest, however, it appears again. Irritation in the region of the cervical sympathetic by means of moist electrodes and the faradic current, also calls forth dilatation of the pupils. It has not been possible for me to decide, as yet, whether this occurs through direct irritation of the sympathetic, or in a reflex manner from the skin or deeper tissues. In patients with tabes, on the contrary, presenting reflex immobility, and usually myosis also, all reactions of the pupils are absent. With the strongest possible irritations of the skin to an unendurable degree with faradic brushing, as well as the sharpest pinching, and even with direct irritation in the region of the sympathetic with moist electrodes, not a trace of pupillary dilatation appears. I have also had an opportunity of examining a patient with tabes having the most marked spinal myosis with reflex immobility, at a time when he was being tortured by the most fearful neuralgic pains of the intercostal nerves and the lower extremities. During the attack of pain the pupils remained as before, not showing the slightest change. On the other hand, in cases of tabes with preserved pupillary reaction—and in these myosis is usually absent—dilatation of the pupils follows cutaneous excitation as in health.

I have not been able, thus far, to examine sufficient material to decide whether this non-dilatation, after cutaneous

irritation, is more closely connected with myosis, than with reflex immobility. This will be the next problem for investigation. So much, at least, follows from my experiments, that in tabes, there is not merely an abolition of reflex *contraction* of the pupils, but often an abolition of their reflex *dilatation.*

As until now these two phenomena appear to have been regularly associated, the title, "reflex pupillary immobility," chosen by me, is, in a double sense, correct. It applies to the reflexes from light, as well as to the skin reflexes; to the reflex contraction, as well as to the reflex dilatation of the pupils.*

The question whether these facts make a true anatomical and functional relation between (*sit venia verbo!*) *contraction* and *dilatation centres* for the pupils probable, will be left to the discussion of physiologists, and for decision by means of future experiments.

In other forms of disease I have made similar experiments. In a case of Basedow's disease without abnormalities of the pupils, dilatation appeared on cutaneous irritation. In a case of bilateral mydriasis with complete pupillary immobility, and with paresis of accommodation, probably dependent upon syphilis, all reaction from the skin failed at first, but, after a time, the contraction of the pupils to light returning, the dilatation on cutaneous irritation again appeared. In another similar case (No. 3), formerly syphilitic, with, on the left side, very marked myosis and reflex immobility, and on the right, mydriasis, with reflex and accommodative immobility, irritation of the skin failed to develop the slightest trace of reaction. On the other hand, in one case of unilateral amaurosis from neuritis retro-bulbaris, in which, on this side, reaction

* One may speak, therefore, of abolished pupillary reflexes. Is this not analogous, probably, to that constant and important symptom in tabes, the absence of the tendon reflex?

to light failed completely, slight dilatation, equal on both sides, was obtained by faradic brushing.

I would consider it entirely too precipitate to draw any conclusion from these few facts, respecting the complicated mechanism for the innervation of the iris, and the more exact nature and localization of the lesions in tabetic myosis with reflex pupillary immobility. These facts are only intended as the very beginning of a more exact investigation of the interesting relations of the pupils in tabes, and in other diseases.

It would be easy to fill many pages with theoretical speculations concerning the various possibilities which might be taken into consideration, in the highly intricate mechanism for the innervation of the iris, and in the pathology of the doubtful pupillary phenomena as well.

It is far from my intention to apply these considerations here, which really could have no other use than to suggest a search for, and the determination of, new facts. Such suggestions will force themselves upon whomsoever will earnestly occupy himself with this interesting subject.

First of all, it appears to me necessary to collect more facts, and to subject the normal as well as the pathologically changed pupils to still further variously modified experiments, in order to obtain a deeper insight into the nature and individual conditions of their disturbances.

It must be determined, first, according to my opinion, how the pupils behave in myosis with preserved reaction to light, as, for example, in the aged, and in many healthy persons; further, how they react in artificial myosis produced by calabar; then, whether with tabes with reflex immobility but *without* myosis, the case is different from that *with* myosis, and whether, also, the reflex excitability from the skin only fails in preserved reflex immobility; or, again, whether it has anything more to do with myosis; finally,

this question will be suitable for an experimental investigation on animals, in which, through Bessau's dissertation, a beginning worthy of attention has been made.

After further manifold studies we will finally be in a position to estimate more exactly the meaning of the pupillary phenomena treated of here in certain disorders of the central nervous system, especially in tabes dorsalis ; but to.day, also, this symptom has a diagnostic worth not to be undervalued. It will aid in many cases for the more certain differentiation between beginning tabetic disease, and purely functional disorders, as, for example, neurasthenia, hypochondriasis, etc.

BIBLIOGRAPHY.

1. Argyll Robertson. *Edinb. Med. Journal*, 1869.

2. Virchow-Hirsch's Jahrsber., 1872, ii, s. 544.

3. Wernicke. Über d. Nachalten d. Pupillen bei Geisteskranken. *Virch. Arch.*, Band 56, 1872.

4. Hempel. Ueber Spinalmyosis. *v. Graefe's Arch. f. Ophthalmol.*, Bd. xxii., 1, 1876.

5. Vincent. Des phénomènes oculo-pupillaires dans l'ataxie locomotr. progress. et la paralysie générale des aliénés. *Thése de Paris*, 1877.

6. W. Erb. Zur Pathologie der Tabes dorsalis. *Deutsch. Arch. f. Klin. Medicin*, Bd. 24, s. 1, 1879.

7. W. Erb. Die Beziehungen d. Tabes dorsalis zur Syphilis. Tageblatt d. 52. Versaml. deutsch. Naturforschen u. Arzte in Baden-Baden, 1879, s. 312.

8. Kahlmann u. Witkowski. Über das Nachalten der Pupillen wahrend des Schlafs nebst Bemerk. über die Innervation der Iris. *Arch. f. Anat. u. Phys.* Phys. Abtheil., 1879, s. 109. Also,

Sander. Ueber d. Beziehungen der Augen zum wachen. u schlafenden Zustand des Gehirns, etc. *Arch. f. Psych. u. Nervenk.*, Bd. ix, 1878.

SOME PRACTICAL REMARKS ON CHRONIC RHEUMATISM.*

By WILLIAM PEPPER, M.D.,

PROFESSOR OF CLINICAL MEDICINE IN UNIVERSITY OF PENNSYLVANIA.

AS the remarks I propose to make to-day are purely of a practical character, I shall be able to avoid all discussion of disputed theoretical points. Nor have I any idea of attempting a description of all the varieties of chronic rheumatism so-called, or a recital of all the innumerable remedies recommended for their treatment. My only object is to try to sketch a few of the groups of cases that come most frequently under my observation, and to mention the practical lessons I have learned from their study.

In the first place, while it is undoubtedly true that some cases of so-called chronic rheumatism are really rheumatic in nature, it is equally certain that others are not so. It seems to me too much the custom to call a case one of chronic rheumatism simply because the seat of the disease is in one or more joints. It is true we are entirely ignorant of the essential nature of rheumatism, but at least all agree in considering that it is a general or constitutional disease, and that the articular affections are merely its local manifestations. On the other hand, it cannot be doubted that the different tissues entering into the formation of a joint are liable to various other sorts of diseased action. Gouty

* Read before the Oxford Medical Society, July 21, 1880.

or syphilitic inflammation may occur; traumatic irritation is very common, and simple idiopathic inflammation from ordinary causes, such as damp, changes of weather, etc., may occur here as elsewhere. In addition to this, it would be found on careful examination, I am sure, that many painful affections of tissues near joints are commonly called rheumatic, although the joints proper are themselves not involved, unless secondarily.

Among the cases that may be selected in illustration of these remarks, I have met with a strikingly large number where the shoulder joints have been affected. The symptoms with which such patients have presented themselves have been pain referred to the shoulder joint, and inability to make the ordinary movements of the arm, particularly to raise it above the head or to put the hand behind the back.

In the majority of cases only one shoulder is affected, but I have met with a good many instances where both were involved. Sometimes the affection has been of a rheumatic character, and either originally formed part of a general articular disease, or was from the first the only local manifestation of a constitutional disturbance. But frequently no such rheumatic element could be assumed positively; but the affection appeared purely a local one. The cause has occasionally been traumatic,—as a fall, striking on the open outstretched hand, or so as to drive the head of the humerus violently against the glenoid cavity, or so as to put on the stretch and irritate the synovial capsule and the nerves which pass in close proximity to the joint. The same result has been caused by a blow on the shoulder, or by a sudden and violent muscular exertion. In other cases, a sudden chilling of the surface while overheated, as by a cool draught blowing on the shoulder, has excited the inflammation. Sometimes the trouble has come on very in-

sidiously, by the repetition of trifling and almost unnoticed irritations, until finally a state of positive disease is established. But, however excited, the anatomical conditions and the symptoms are similar.

Pain is, as I have said, a constant symptom. It is often worse at night, and interferes much with sleep. It is of a wearing, sickening character, and is increased by attempts at motion or by allowing the arm to hang downward. The head of the humerus quickly assumes the position of a slight subluxation forward and downward. Pressure with the finger along the course of the brachial plexus constantly reveals decided local tenderness of the nerve trunks, due to a neuritis or perineuritis that has resulted either from the original cause of the attack, or from an extension of inflammation from the synovial capsule. In case the head of the humerus is allowed to occupy its unnatural position, the irritation of the nerve trunks is greatly increased by the pressure of the head of the bone, and the secondary neuritis becomes more serious and extensive. The circumflex and the median nerves are those most commonly involved. The pain now radiates along the course of these nerves and especially extends down the arms and into the fingers; a feeling of numbness and tingling, or burning, is apt to accompany it. Liquid effusion into the synovial capsule is rare, but a tendency to adhesive inflammation rapidly shows itself, and in a wonderfully short time slight, false anchylosis develops, which, if neglected, grows more and more close and firm, holding the head of the humerus with constantly increasing force in its abnormal position. From the very first, the power of movement of the arm is much impaired. The hand and forearm do not share in this; but the arm can be lifted only a short distance from the side, so that the hand cannot be fully elevated. Rotation of the humerus is prevented so that the hand cannot be carried behind the

back. The patient soon finds he cannot use the hand on the affected side either in eating or in dressing.

The angle to which the arm can be raised varies greatly in different cases, and in testing the power of motion it is necessary to guard against error by fixing the scapula by firm pressure, since otherwise the patient will unconsciously deceive by tilting the thorax toward the sound side, and thus apparently bringing the humerus on the affected side to a higher level.

The cause of this impaired mobility is at first the instinctive avoidance of pain and the lessened power of the deltoid from the irritation of the circumflex nerve, which is distributed to this muscle. But other influences soon come into play. Adhesions form and hold the head of the bone more and more firmly; the deltoid soon begins to waste from disuse, and the inflammation of the circumflex nerve impairs the nutrition of the muscle and hastens its atrophy. The dependent position and disuse of the arm, and the interruption of circulation caused by the pressure on the veins, lead to passive congestion and œdema of the hand and forearm. If a descending neuritis is established in the ulnar and median nerves, atrophy of the muscles of the forearm and hand ensues ere long, and, finally, a most helpless condition of the member is brought about.

Brief notes of a few illustrative cases may be interesting:

Case 1.—Mr. S. R. S., æt. 50, banker, fell on ice, and holding out left hand to save himself, felt a sharp pain in left shoulder joint. When seen, two days later, there was inability to lift left arm more than to angle of 45° from body, and that was very painful. There was exquisite tenderness over the brachial plexus; the circumflex nerve felt swollen, and was especially painful. The arm was supported so as to carry upward and backward the head of the bone. Passive motion was begun at once, and continued

daily so as to prevent adhesion; a blister was applied over the inflamed nerve trunks; iodide of potassium and bichloride of mercury given internally. In a few days the soreness was greatly relieved, and then faradic electricity was applied daily to deltoid; more thorough passive movements were practised, and a very rapid cure followed.

Case 2.—George Kiegel, æt. 57, carter, came to me on April 24, 1877. The month previously began to notice pain in right shoulder; shortly before that had fallen on the ice. The aching pain continued, and in two weeks he had to give up work, and in two weeks more he could not dress himself. The case had been regarded as one of rheumatism. The arm had been kept quiet, and internal and local remedies used. The pain and helplessness steadily increased. When I first saw him, he could not lift humerus from side at all. The deltoid was markedly atrophied. The head of the humerus was in advance of its normal position, and was very firmly held there by strong adhesions. There was intense tenderness with marked swelling of the nerve trunks in front of the head of the humerus. There was severe pain about the shoulder, also extending down the arm along the course of the nerves, most acutely felt at elbow and at interphalangeal joints. This latter pain was much increased by closing the hand. There was numbness of hand and arm, and occasionally the hand became swollen. Unquestionably storms and sudden changes of weather increased pain, numbness and weakness. With dynamometer right hand gave only 40; left hand 110. Iodide of potassium, gr. v, t. d.; repeated blistering over the inflamed nerve trunks; persistent graduated passive movements until adhesions were all stretched and broken, and then faradisation of deltoid and shoulder group of muscles, constituted the treatment. By June 12, 1877 (seven weeks), the arm could be moved passively in all directions; the deltoid was regaining its size and power, and power of movement had returned to a great extent, although he was not yet allowed to work. R. hand with dynamometer 100. Pain entirely relieved.

Case 3.—Mrs. M., from Chester, Penn., æt. 45, accustomed to doing rather laborious housework, applied to me in 1875 for the relief of extreme pain in the right arm, combined with total helplessness. She had evidently taken cold repeatedly while overheated, and had had repeated slight attacks of pain about the right shoulder joint. Finally the pain grew so severe that she was obliged to give up work, and soon she found herself unable to

lift her arm from her side, or lift her hand to her head to feed herself, or to use it in dresssing herself; and finally it grew almost entirely useless. During this time she suffered constant pain. It was of a wearing, dull character, in the shoulder joint, frequently shooting down the arm to the fingers, and was so severe at night that she scarcely slept at all; and her general health had suffered greatly, with much loss of flesh in consequence. The condition had lasted for several months when she first consulted me. She stated that she had been treated for chronic rheumatism, and that she had been recommended to keep the arm at rest. There was advanced atrophy of the right deltoid muscle, and the application of the faradic current, or of a slowly interrupted galvanic current, caused scarcely any contraction of its fibres. The head of the right humerus was firmly fixed in a position of slight subluxation forward and downward, and any attempt to rotate it, or to elevate it in any direction, met with firm resistance, and caused intense pain. The cords of the brachial plexus were swollen, hardened, and exquisitely tender. The muscles of the right arm and forearm were somewhat atrophied; the hand was puffy and swollen, and there were severe complaints of burning and tingling pain, with numbness down the arm and through the hand. Systematic manipulation of the arm, directed toward breaking up the adhesions, was used at intervals of about five days, despite the intense suffering caused. After each treatment, however, the pain was lessened, and mobility was increased.

She was also exhorted to use the arm as much as possible, carrying her efforts as far as her endurance would enable her to do. Repeated blisters along the inflamed nerves were used; the whole shoulder was enveloped in a batt of wool saturated in a strong liniment of chloroform and aconite. Iodide of potassium and bichloride of mercury were given internally. After some degree of mobility was restored, massage of the deltoid, with occasional faradisation, was used.

Electromotor contractility gradually returned, and the muscle gained in bulk satisfactorily. Treatment extended over three months, by the end of which time pain was entirely relieved, and she was able to use her arm quite freely. She was directed subsequently to continue regular gymnastic exercise with it, so as to thoroughly complete the restoration of motion and power, and I learn now (June, 1880), that the arm has long since returned to its normal state.

I could quote from my case-books the records of a very large number of instances presenting the same essential conditions as the last; but I will only tax your patience by reading the notes of one of a different type, although illustrating some of the same points.

Case 4.—Mrs. R. A., sent to me Sept. 4, 1875, from Belmont County, Ohio. She was about 40 years of age, and had enjoyed general good health. Two years previously, numbness in both hands came on quite suddenly, and gradually grew worse, extending up arms to shoulders. There was gradually increasing weakness of arms, and frequent aching pain, especially before changes in the weather, in shoulders and down arms. The case had been regarded as one of chronic rheumatism; she had been directed to keep the arms as quiet as possible; liniments had been used and anti-rheumatic remedies given internally. The left arm was the worse. The deltoid was considerably atrophied, and quite close anchylosis of the shoulder joint existed. The muscles of both arms, especially the left, were decidedly atrophied.

Unquestionably the beginning of this interesting case was a rheumatic neuritis of the nerve trunks (medians or ulnars) in both arms, which ascended until it reached the circumflex nerve and the brachial plexus. Partial loss of power of the deltoids (particularly the left) had combined, with intentional disuse, to allow the head of the humerus to remain comparatively motionless, until adhesions formed between it and the glenoid cavity.

Passive movements until all adhesions were broken up; regulated massage and exercise; persistent counter-irritation along course of inflamed nerves; the use of the constant galvanic current, and the internal use of iodide of potassium with small doses of bichloride of mercury, was the treatment directed. The patient returned home at once, and the result is unknown.

It is not only in reference to affections of the shoulder joint that a careful study of the adjacent nerve trunks is important, but I know no other joint where arthritis is so apt to be associated with neuritis. Sometimes the neuritis is the primary trouble, and the joint becomes involved secondarily; more commonly, the arthritis precedes and a secondary neuritis from extension of irritation ensues.

The essential point, however, is to recognize the two elements and to adapt the treatment accordingly. If the case be seen at an early stage, before any anchylosis or atrophy of the deltoid has resulted, a rapid cure can be effected by the use of a suitable bandage to support the arm and carry the head of the humerus upward and backward, thus obviating any pressure on the nerves or vessels; by active counter-irritation along the course of the nerve trunks if they are found tender and swollen; by the internal use of full doses of quinia, together with iodide of potassium and bichloride of mercury; and, as soon as the acute inflammation is subdued, by the application of a galvanic current, the positive pole being placed over the affected nerves and the negative pole over the deltoid muscle.* But while these measures are being carried out, it is essential that, as soon as the acute stage has passed by (say after the first two or three days), gentle and gradually increased passive movements of the arm should be practised.

But, in my own experience, such cases have much more commonly come under observation at a later stage, and when more or less serious changes have occurred. The first point of importance, then, is the diagnosis, and there are several conditions with which it is possible to confound the affection we are considering. In the first place, finding the head of the humerus somewhat displaced from its normal position, the shoulder decidedly flattened, and the movements of the arm much restricted and painful, and learning possibly that some fall or twist had preceded the trouble, the idea would naturally occur of a subluxation of the humerus. Indeed, as I have already said, there does come

* We owe to Remak the demonstration of the great value of the constant current thus applied in cases of articular neuritis, accompanied with paresis of the deltoid. See his noteworthy "application du courant constant au traitement des névroses," Paris, 1865, pp. 41. Extracted from *Revue des Cours Scientifiques.*

to be a slight degree of subluxation, and in very chronic cases where immobility has been allowed to continue for a long time, the glenoid cavity undergoes such changes as to render it impossible for the head of the humerus ever to resume its normal position. But I have known cases where, after the acute stage had been injudiciously treated with the usual result of anchylosis, the patient has fallen into the hands of unscrupulous charlatans or ignorant bone-setters, who have been shrewd enough, however, to recognize the necessity of forcible motion of the joint, and then, on finding, after one or two sharp cracking sounds have been distinctly perceived, that the bone returns more nearly to its normal position, and that marked improvement in the power of movement has ensued, have advanced the theory that the case has been one of neglected subluxation from the first, and that damages for malpractice should be claimed. Such an error would be impossible at the early stage of the case; and later, by careful attention to the history and evolution of the case, and by observing that the restriction of mobility is not only in the direction that would result from a subluxation forward, and that just in proportion as the adhesions are broken up by gradual passive exercise, normal mobility returns, it is possible to avoid any mistake.

I have repeatedly known such cases to be regarded merely as paralysis with atrophy of the deltoid, and a treatment of persistent faradisation, hypodermic injections of strychnia, etc., to be carried out, but, of course, without any result, because the cardinal fact was overlooked that the paralysis and atrophy of the deltoid (which undoubtedly existed) resulted from: 1st, neuritis of the circumflex nerve, excited and maintained by the articular trouble and the abnormal position of the humerus; and 2d, by disuse owing to neglected anchylosis of the shoulder joint. The mode of

development of the case, the early impairment of motion in directions not requiring the action of the deltoid, the pain and tenderness, and, finally, the anchylosis,—all render easy the recognition of the true nature of the case, and show that the conditions of the deltoid are purely secondary.

The mode of treatment that succeeds, even in very bad and long continued cases, has been, perhaps, sufficiently alluded to in the brief records of several cases already given. A few words may be added, however, in regard to some of the points.

Systematic passive movement and massage are the most essential parts of the treatment. Without these to free the head of the bone from its abnormal position and relieve pressure on the nerves and vessels, all else must fail utterly; while just in proportion as the adhesions are broken up, all other symptoms improve. I may say that, in my experience, etherization and the forcible breaking of the adhesions has never resulted as favorably as their more gradual destruction by repeated, comparatively gentle passive movements. Used with the utmost care, however, great suffering is always caused; but this must be disregarded, and the manipulations be steadily persisted in, since all delays increase the atrophy of the muscles and render a cure less hopeful. In addition, the patient should be encouraged to use the arm as much as possible, instead of being allowed or directed to keep it quiet.

Pain is often intense. I have frequently known the general health suffer gravely from the interference with sleep. The removal of the adhesions is the surest mode of affording relief, but more immediate methods are needed. The hypodermic injection of morphia and atropia, the application of strong veratria ointment, or of a strong liniment of aconite and chloroform, and the application of the constant galvanic current are the most prompt and reliable remedies.

Neuritis is so common a complication that the condition of the nerve trunks (especially the circumflex and the median and ulnar) nearly always calls for treatment. Repeated small blisters along the course of these nerves, and the use of the galvanic current in the way already mentioned, *i. e.*, with the positive pole over the irritated nerve trunk, and the negative pole over the fibres of the deltoid, is the best treatment, combined with the internal use of iodide of potassium with small doses of mercury. I should not limit the use of these remedies to those cases only where a rheumatic element clearly exists, but would recommend their use generally for the relief of the chronic arthritis and of the inflammatory swelling of the nerve trunks.

As a further illustration of an analogous form of articular and neural trouble, although involving a different member, the following interesting case may be cited:

Case 5.—Mr. D., of Mahanoy City, age 38, has never had syphilis, but has been much exposed to wet and cold. In August, 1877, began to suffer with pain and swelling of the right knee, and with pain in the muscles of the back, and extending thence around the limb, and down the front of the right thigh as far as the knee. On August 18th the pain became very violent, and he was confined to bed for five weeks; since then it has not been so violent, though subject to frequent, severe, sudden shocks. There has also been constant pain about the right knee and along the thigh. He consulted me November 1, 1877. The right knee joint was swollen, with some effusion, painful and stiff. The anterior group of thigh muscles were weak and decidedly wasted. Marked tenderness existed along the course of the anterior crural nerve. The urine presented a heavy deposit of urates. A plaster of ammoniac and mercury was applied over the knee; blisters were applied along the course of the affected nerve; mild faradisation of the muscles of the thigh was employed daily; and internally iodide of potassium, three grains, increased to six grains, with Donovan's solution, ten drops, thrice daily, were given. In the course of two weeks improvement was marked, and went on steadily to complete recovery. When last seen, in September, 1879, he remained perfectly well.

I have dwelt thus minutely on this form of articular trouble, not only because I am led to believe it is a rather frequent affection, and one which does not always receive prompt recognition and appropriate treatment, but because it seems to me to illustrate clearly the important points: that painful articular affections are by no means always of a rheumatic character; that many of the symptoms connected with articular affections may be due to implication of surrounding tissues, and particularly to inflammation of adjacent nerve-trunks; that the paralysis and atrophy of muscles connected with the affected part, which constitute most serious complications, are often attributable to the influence of a neuritis, more than to that of prolonged inaction; that in many cases of arthritis without liquid effusion there is a strong tendency to the formation of adhesions; and that this false anchylosis, particularly where, as in the shoulder joint, the bone becomes fixed in an abnormal position, constitutes a serious complication and should be prevented by an early resort to gentle, systematic passive movements.

In connection with this most important and difficult question of when to use rest and when to use movement in the treatment of articular affections, I will allude briefly to the subject of rheumatoid arthritis. It is clear that this interesting disease has no direct relations with either acute articular rheumatism or with true gout. But it has unmistakable analogies with conditions that must be grouped under the name of chronic rheumatism. It attacks persons who, whether from inherited tendencies or from acquired weakness, present conditions of depressed vitality with abnormal sensitiveness to the action of damp and climatic changes. In my own experience, by far the most common and demonstrable cause has been residence in a damp house or a damp locality, operating on a system enfeebled

by such depressing influences as excessive child-bearing and sexual exhaustion, or as a severe prostrating illness, such as typhoid-pneumonia. There seems to be developed gradually, perhaps in consequence of the defective action of skin caused by the prolonged action of damp or by repeated chillings of the surface, a morbid state of the synovial tissues, and to a greater or less degree of the subjacent articular cartilages. One joint after another becomes involved, with a certain regard to symmetry, but without regard to the size or locality of the joints. Some effusion occurs at first into the synovial capsules, but later this is apt to be absorbed; the synovial membrane is thickened and roughened; in places destruction occurs both of the membrane and of the subjacent articular cartilage. Meanwhile the margins of the joints are involved, and ridges or nodules of new-formed bony tissue appear; the fibrous tissues become thickened, and the tendons no longer play freely through their sheaths; the whole joint becomes more and more distorted and useless; motion grows more and more painful and difficult; finally, firm anchylosis occurs with great deformity, and the functions of the joints are utterly destroyed. One point of very great practical importance in connection with these cases has not been sufficiently noticed. It is the fact that the subacute inflammation extends from the surrounding fibrous tissues to the sheaths of the nerves, and an ascending and descending neuritis is apt to be set up. Not only does this complication cause a serious addition to the suffering in the form of pain radiating along the course of the affected nerves, but it induces grave nutritive changes, such as a more rapid and extreme atrophy of the muscles than would result from mere disuse, and even degenerative lesions of the skin and nails. The terrible state of helplessness to which the unhappy victims of rheumatoid arthritis are brought, in the later stages of

the malady, is familiar to you all, and is well illustrated by this sketch of a patient now in the ward of Philadelphia Hospital, for which I have to thank the skillful brush of my friend Dr. J. M. Taylor.

It is needless to say that by the time a patient reaches any such condition as this, he is far past all hope of real relief from medical treatment. But in the earlier stages I have had numerous occasions of late years of seeing what vast improvement can be effected by systematic treatment of a certain character. I began the study of rheumatoid arthritis with the idea that it was a hopelessly incurable disease. I was aware that every remedy in the pharmacopœia had been used without positive benefit in its treatment. I can add my testimony, after prolonged and faithful trials, that little is to be expected from the use of any of the well-known antirheumatic remedies in rheumatoid arthritis. And yet I have become satisfied that in many cases—and I mean to include severe cases and quite advanced stages of the disease—vast relief can be afforded to the symptoms, the progress of the disease can be checked, and even a considerable degree of usefulness be restored to badly crippled joints.

The notes of the two following cases may be cited here as illustrative, both of the symptoms of rheumatoid arthritis and of the general features of the plan of treatment to which I beg to call your attention:

CASE 6.—Mrs. C., living near Woodbury, N. J., came under my care in March, 1878. There had been no rheumatism or gout among her grandparents, uncles or aunts. Her father died of palsy at 72, her mother of puerperal fever at 39. She had herself one sister who began to have arthritis at three years of age, and ended by having her joints greatly distorted; finally the disease became inactive, and she lived to the age of 38 years, when she died in childbirth. Mrs. C. herself always enjoyed good health. She was always very sensitive to changes of

weather, and required a great deal of clothing. She ceased menstruating at the age of 49. About the same time she was subject to great mental distress. While passing through the menopause she noticed subacute inflammation of the right great toe, which soon spread to the joints of other toes, and then invaded the wrists and hands, then the knees, then the hips, and at last extended to the elbows and shoulders ; the ankles alone were never much affected. The pain in the affected joints was very violent, especially at night ; it was apparently influenced by atmospheric changes. It was chiefly seated in the affected joints, but there were also at times lancinating pains along the members. There was marked wasting, with great loss of power of the muscles connected with the affected joints.

There was marked deformity of many of the joints, of the character typical of rheumatoid arthritis. She did not suffer with headache and there was no spinal tenderness, but there was pain in the lower part of the spine. She was confined to a rolling chair. She could move her arms at the shoulders pretty well, but the elbows were very stiff and the wrist rigidly fixed. The legs were fixed at a right angle so firmly that great force was required to elevate them, and motion caused extreme pain. There was marked wasting of the extensor muscles of the thighs, with great tenderness on pressure along the course of the nerves and at certain points along the shafts of the femurs. There was no cardiac disease.

The patient was removed to the University Hospital, where she remained for thirty days, and then returned to her home in Woodbury. Systematic manipulation of the affected members was practised daily, with forcible movements of the stiffened joints. This movement was effected partly by skillful massage, partly by various mechanical appliances. She was especially urged to make regular muscular exertions herself, and to use the appliances provided to effect as much motion of the joint as possible. A considerable portion of every day was consumed in this regular gymnastic work. At first very limited motion only was possible, but gradually all the joints except the wrists yielded and became much more movable. Regular daily frictions of the whole surface with oil, and twice weekly with alcohol, were also used ; extreme care in dressing was urged, as also in regard to exposure to draughts or to sudden changes of temperature.

Thorough treatment by faradisation of the whole muscular system was carried out on alternate days. Internally she took nitrate

of silver and small doses of opium and belladonna, given in pill form, thrice daily, until 30 grs. of the silver salt had been taken; and, after an interval of three weeks, another course of 20 grs. was given. Dialyzed iron in doses increasing up to one fluid drachm, thrice daily, was given steadily for a long time. In the intervals between the courses of nitrate of silver she took iodized cod liver oil.

When she left the hospital she was acquiring gradually increasing power of motion in her joints, and the wasted muscles were gaining in size and strength. She had begun to walk with but slight assistance. Subsequently the same line of treatment was carried out with most gratifying results. The pain was almost entirely relieved, her general health became much better, and at the last time I heard from her she was able to move her joints even more freely than at any previous time of her sickness.

I will also give a brief abstract of the notes of the following interesting case, because it was an example of great benefit obtained in an apparently hopeless state (and because the patient may perhaps be well known to some of my hearers).

Case 7.—Mrs. S., of Wakefield, Lancaster Co., Penn., lived in a house which was probably quite damp, and at the age of 27, while feeling somewhat run down, she had a severe attack of pneumonia of the left lung, ten days after which she was taken with inflammation of the left shoulder and elbow, soon followed by inflammation of both knees and hips. The pain was very acute even during entire rest, especially so at night, and was much increased by motion. Since then the disease had progressed with acute exacerbations at irregular intervals, at which time new joints became affected, and those already diseased became worse. In the intervals the acute symptoms would subside to some degree. At the time of the exacerbations there was usually some fever. The urine had frequently shown deposits of urates. The general health suffered severely: there was great loss of flesh, color and strength; sleep became very poor, being disturbed by pain, and also by painful contractions of the flexors of the lower extremities. During several months previous to my first seeing her, these contractions were especially severe. There was tenderness over one point of the lumbar spine, but no evidence of

spinal disease. The habit of using morphia freely had existed for a considerable time. On admission to the University Hospital, October 23, 1877, the disease having lasted six years, Mrs. S. was almost entirely helpless, at the age of 34. She was confined to her chair, being entirely unable to straighten herself or to stand. There were stiffness and impaired motion of the cervical spine. There were excessive pain andt enderness of the affected joints, all of which presented the most characteristic changes and deformities of rheumatoid arthritis. These consisted of swelling and deformity; within some a moderate amount of effusion in the synovial sacs, and in others stiffness or firm anchylosis; lesions of the synovial membrane and articular cartilages as shown by crackling and crepitus on motion; and, finally, new bony formations around the margins of the affected joints. There was advanced wasting of the muscles nearly all over the body. An imperfect response was given to faradic currents. Pain was extreme, and although her endurance was heroic, the nervous system was considerably disturbed. There were extreme pallor and marked emaciation, the weight being only 104 pounds. Not only were the legs immovably fixed, but the arms were likewise crippled so that she could not feed herself; and the hands were entirely useless. The contractions of the legs, already noted, were very marked and painful. They came upon her frequently and without any cause, although effort would always provoke them. Owing to the stiffness of the knee-joints they did not cause very much drawing of the legs upward, but the flexor muscles could be seen or felt to contract in a sudden spasmodic manner, and then to soon relax more slowly and irregularly. In addition to these large muscular contractions, sudden jerking movements of the fingers were noticed. Even those which were dislocated in consequence of the articular changes, would be suddenly seized with jerking, and would shake rapidly and uncontrollably. Fibrillar contractions of the muscles of the hands and of the forearms had been noticed for a year past.

Her diet was very carefully regulated, and she was encouraged to take largely of light nourishing food, in addition to considerable amounts of milk.

A pill of nitrate of silver, gr. ¼, extract of opium gr. ¼, extract of belladonna gr. ⅛, t. d. p. c., was ordered. Daily inunctions of oil, with massage of the whole muscular system, and systematic manipulation of all the affected joints, were faithfully kept up.

Mechanical appliances were devised to gradually break up the

adhesions of the larger joints, and arrangements were made by which, as soon as a little movement in any joint was secured, systematic exercise of the muscles attached, could be maintained. The muscles were carefully and gently faradized.

In the course of a month there had been considerable improvement. The appetite was better, and she had commenced to gain much flesh. There was less tendency to exacerbations, and her pain was diminished, so that she could sleep fairly well without morphia and with diminishing doses of opium in her pills. The joints had yielded to manipulation better than seemed possible, so that movement was returning in many of them, and their enlargement was diminishing. The muscles responded better to electricity. Spasmodic contractions of the legs at night continued, but this was relieved by bromide of potassium. She began to take dialyzed iron about December 1st, and took it for many months : it was impossible, however, to give it to her in larger doses than fifteen to twenty drops, thrice daily, for when increased beyond this it caused looseness of the bowels. She took 40 grains of nitrate of silver continuously, then stopped it for several weeks, and resumed it, taking 15 grains more ; and this she repeated until she had taken in all about 75 grains while at the hospital. Her improvement was slow but steady ; by February 1st her weight had increased to 114 pounds. Marked improvement had occurred in the power of motion, and in the anatomical condition of the joints. She became able to help herself in many ways, and finally to walk about with the assistance of the Darrach wheeled crutch. She left the hospital in the summer of '78, to return to her home, where the same treatment was to be carried out.

By December 15, 1879, her weight had increased to 140 pounds,—a gain of fully 36 pounds since she first came under my care.

She was almost free from pain ; her functions were all well performed, and there was great improvement in the mobility and power of motion of nearly all the joints. She was able to walk considerably with the apparatus named, and could also run a Howe's sewing machine herself. Massage and inunctions had been steadily continued, and faradisation had been used occasionally.

She had been taking dialyzed iron constantly for over two years, and since leaving the hospital had used 75 grains of the nitrate of siver in the course of eighteen months, making 150 grains in all.

While some of the joints remained anchylosed, or distorted, in consequence of the advanced lesions that had been developed previous to her coming under my care, it may safely be said that the improvement in this interesting case was most gratifying and encouraging.

In these cases, and so in all similar cases, where I have succeeded in effecting any material relief, one of the most important, or probably the most important element in the treatment has been systematic daily manipulation. This includes persistent movement of all the affected joints, excepting those where anchylosis has been allowed to become so firm that any motion is impossible. But even when complete immobility has apparently been established, I have frequently been surprised to find that vigorous efforts have restored some measure of usefulness to the part. In the case of joints where the inflammation presents a very acute stage, attended with rapid swelling and decided heat and redness, it is proper to await the subsidence of this severe irritation before instituting regular manipulations; but the delay need rarely be long. Of course such manipulations are excessively painful, and must be conducted daily without anæsthesia. Still, so highly important do I consider this treatment, and so excellent are the results often obtainable by it, and by it alone, in cases which otherwise would pass steadily into more and more settled helplessness, that I feel no hesitation in appealing, and always with success, to the patient to undergo it thoroughly and persistently. Where the tendency to contraction, deformity and anchylosis is too great to be overcome by ordinary manipulation, suitable apparatus may be contrived to assist in overcoming it, as was done in Case 7.

I would beg to repeat, then, that in rheumatoid arthritis, from an early to a late stage, despite the pain occasioned by such manipulation, I consider the most essential part of treatment to be the systematic daily movement of the

affected joints, combined with thorough massage of all the muscles whose functional activity is impeded and impaired. It is not, indeed, alone the maintenance of the mobility of the joints that is arrived at in such treatment. The circulation of the tissues around the joints is stimulated, and the tendency to absorption of the exudation is increased. The nutrition of the muscles is maintained, and the atrophy of their tissue, that we have all had occasion to note as among the most serious results of this form of joint disease, is as far as possible obviated. But in addition to all this, there seems every reason to believe that in this affection, as in nearly all cases of chronic rheumatism also, there is an underlying impairment of the tone and activity of the skin which is the strongest predisposing cause. There is no way in which this can be improved so well as by systematic manipulations and frictions, accompanied by the use of suitable baths, or by inunction with a vegetable oil. In feeble, anæmic rheumatic patients, where even hot salt baths of very short duration may be badly borne, or, on account of the crippled joints, may be inconvenient, the thorough daily inunction of the whole surface with pure olive or cocoa oil has for a number of years been a favorite practice with me.

In many such cases change of residence, and, if possible, change of climate is extremely beneficial. The diet nearly always requires close attention, and, as a rule, it is necessary to arrange it so that a much larger quantity than formerly shall be taken of simple, wholesome food, which is often best done by adding two or three pints of skimmed milk per diem to the regular diet.

It has long been recognized that patients with rheumatoid arthritis usually present an anæmic condition, and that nutrients and alterative tonics produce better results than any specific remedies, such as iodine, iodide of potassium,

or guaiacum, etc. Of all tonics, iron in very large doses has proved by far the most valuable in such cases, and in unusually large amounts it forms an almost invariable part of my treatment of rheumatoid arthritis.

In many instances, especially where there has been marked pain extending along the nerve trunks, and perhaps associated, as often happens, with considerable disturbance of the nervous system, the prolonged use of nitrate of silver, with or without minute doses of opium and belladonna, has seemed to exert a favorable alterative effect. There is a constant temptation to resort to the anodyne use of opium in some form, but it need scarcely be said that this should be resisted most uncompromisingly, since there is scarcely any disease in which the opium habit is more readily acquired, more injurious in its effects and more difficult to break off. Local applications (veratria, aconitia, chloroform) or counter-irritation (iodine, small blisters, various mechanical irritants, or, finally, the constant galvanic current) may afford relief to pain.

It is not, of course, intended to say that other drugs are not called for in many cases. Cod liver oil and arsenic may supplant iron in cases where the latter constipates too persistently. Iodide of potassium with minute dose of bichloride of mercury, may replace or alternate with the use of nitrate of silver. Long continued courses of lithia, as a substitute for all drugs of this latter class, have proved serviceable, especially in cases attended with acid dyspepsia and the uric acid diathesis.

Electricity has been alluded to as a means of relieving local pain, but its systematic use enters as an essential part into the treatment of every case—not only for its effect on the muscles, but on the superficial circulation over affected joints, and on irritated or inflamed nerve trunks.

Lastly, a most rigid attention to hygiene is essential, in-

cluding dress, exercise, avoidance of draughts and damp, etc.

This very hasty and imperfect sketch shows clearly enough the well-known truth, that it is not on any one drug or combination of drugs that we are to rely in the treatment of rheumatoid arthritis any more than in other chronic diseases; but that it is only by a thoroughly organized systematic plan of treatment, including hygiene, gymnastics, dietetics and therapeutics, that any success can be obtained. I would beg to emphasize the leading indications in ordinary cases of rheumatoid arthritis, as follows:

To remove the cause—having special regard to residence, soil, moisture, etc.

To maintain at all hazards the mobility of the joints.

To exercise the muscular system.

To restore and maintain the tone of the skin.

To improve the blood and nutrition.

To quiet the pain as far as possible by local means.

To modify the articular inflammation (and that of the adjacent nerve trunks when it exists) by counter-irritation, electricity, and the internal use of alteratives.

It may well be questioned whether such treatment can be successfully carried out under ordinary conditions of home life; and it cannot be doubted but that, for the maintenance of perfect regularity and system in each detail, as well as on account of the skilled attendants and special appliances required, it is generally preferable that such patients should be treated at a suitable institution.

I have spent so much time on the subject of rheumatoid arthritis, that it is not possible to more than glance at some additional practical points in regard to ordinary chronic rheumatism. In the first place, it is clear that in many cases of articular trouble, systematic motion would be injurious, and absolute rest must be enjoined. Here it seems

to me better, not merely to allow the patient to lie in bed, trusting that the limb may be kept comparatively quiet, but to apply either a plaster bandage or a carefully adjusted splint, so as to secure absolute rest conjoined with carefully graduated pressure. The cases that call particularly for this complete rest seem chiefly to be those where the arthritis is very acute and painful, and older cases where there is a considerable amount of liquid effusion in the synovial sac. In the latter cases, no danger of anchylosis exists, and, moreover, the distended and weakened synovial membrane is irritated anew by any strong manipulation or extended movements. In fact, I should be inclined to say, that just in proportion as liquid effusion exists in a diseased joint, is passive movement or active exercise undesirable; while in proportion as the joint is free from such effusion, and presents, instead, thickening, stiffness or adhesions, is manipulation (of course, carefully graduated by the activity of the inflammatory process and the sensitiveness of the part) advisable.

As illustrating the excellent results obtained in chronic rheumatic synovitis and arthritis from rest and pressure, combined with nutrient and alterative treatment, the following case may be cited:

Case 9.—Mr. S., æt. 65, farmer, from Susquehanna County, Penn., was admitted to the University Hospital in 1878. In consequence of working in a damp district, with constant exposure to hard toil, he became gradually crippled with chronic rheumatic inflammation of both knee joints. The shoulders were also affected, but to a less degree. After trying numerous modes of treatment at home and at neighboring mineral springs during the course of several years, and finding that he was growing gradually worse, he went to the Hot Springs of Arkansas, where he used water and baths faithfully, but steadily grew worse, so that he was entirely confined to his bed for several months before being brought to the University Hospital. He was greatly emaciated, anæmic, and feeble. The appetite was poor and digestion

torpid. He was exquisitively sensitive to the least changes of weather, to draughts and damp, so that in every way he presented the highest degree of atony of skin and general system. The shoulder joints were stiff and painful on movement, but there was no liquid effusion therein. Both knee joints were enormously distended with liquid, so that the legs were in slightly flexed condition, and the least attempt at motion caused extreme pain. The synovial membrane was thickened and crackled when moved, and there was some infiltration of the surrounding tissues. The muscles were wasted and flabby. It was impossible for him to stand for a moment even with help of two canes or crutches.

He was kept in bed strictly. The knee joints were enveloped with plaster bandages, which were changed as frequently as the diminishing size of the joints rendered them at all loose. Manipulation of the shoulder joints and thorough massage of the whole surface and muscular system, with inunction, were employed daily. A carefully regulated diet was directed, and he was encouraged to take very full amount of simple, wholesome food. At first he took quinia, strychnia, and muriatic acid, but as soon as the tone of his digestion improved he was put on very large doses of dialyzed iron, with full doses of Donovan's solution, and later of KI. and $Hg.Cl._2$ Very gratifying results followed this treatment. He quickly regained use of the shoulder joints. The effusion in the knee joints steadily subsided, and in the course of three months was so far gone that passive movements were well tolerated. He gained twenty pounds of flesh, improved in color and strength, and in power of resisting changes of weather, and ceased to have exacerbations of pain. He became able to walk with crutches, then with a cane, and before he left the hospital could walk unaided. The muscles of the limbs were developing satisfactorily, and there was every prospect of complete recovery.

I have alluded to the fact that in this instance the thorough use of mineral baths and waters had failed entirely to afford relief, but there are many cases where the proper use of these powerful agents gives us the best possible results. Judging from my own experience, I should say that the cases best adapted to their use are those where the system is not yet too far reduced, so that the power of reaction is not too feeble. It is indeed upon

their power of developing reaction, and thus inducing more vigorous circulation and more healthful secretory activity of the skin, that mineral baths depend for their value in the treatment of rheumatism. It is evident, therefore, that they are to be regarded only as adjuvants, and that at the same time a most careful dietetic, hygienic and medicinal treatment must be carried out. It is largely owing to the universal neglect of this treatment at all American springs that such frequent disappointment awaits rheumatic patients who resort to them for relief. It is not difficult, however, to institute a suitable system of bathing at home, by which many of the good results of this important element in the treatment of chronic rheumatism may be obtained.

We have as yet but little positive knowledge in regard to the essential nature of rheumatism or of gout. All are agreed in regarding acute inflammatory rheumatism as a constitutional disease, although the widest diversity of opinion exists as to its true causes. So, too, there can be no doubt that in many cases of chronic rheumatism, there is the same constitutional disturbance which has assumed the chronic form, either from a repetition of acute attacks or from some peculiar modifying condition that has rendered it chronic from the outset. In such cases there probably exists some defect of primary assimilation or in the action of the great emunctories—liver, kidneys and skin. It is in consequence of this that in so many cases of true chronic rheumatism, great benefit is often derived from careful dietetics—such articles as close observation shows to be digested and assimilated with difficulty being restricted in amount or entirely prohibited. In whatever manner chronic rheumatism may have originated, nothing is more interesting and important than the part which the skin plays in keeping up the disease. So relaxed does the tone of the skin become under the influence of repeated at

tacks of acute rheumatism, or from unfavorable hygienic conditions, that, finally, the most trifling atmospheric changes, a momentary exposure to draught while the body is heated, or many other similarly slight causes, suffice to check the circulation and secretion of the skin and to induce an increase of rheumatic suffering. It would appear, therefore, that any plan of treatment of chronic rheumatism which does not include a most careful attention to the state of the skin must, of necessity, fail in effecting a permanent cure; and clinical experience thoroughly confirms this view. The requirements of individual cases must determine the precise character of this part of the treatment (whether by dry friction, inunction, cold or hot sponge-baths or douches, medicated baths, etc.); but it is sufficient now to request your attention to this as, perhaps, the most important element—though only one of several elements—in the truly curative treatment of chronic rheumatism.

I have thus far made scarcely any allusion to the large group of valuable remedies—mostly of an alterative character—that have acquired reputation in the treatment of chronic rheumatism.

If it is true that in no case of this kind can we afford to depend solely on the use of any of these remedies, to the exclusion of baths, massage, diet, hygiene, it is no less true that in nearly every case there are indications that call for the use of some one or more of them. It would be impossible to discuss at length the merits of the very numerous remedies of this class, so that I must limit myself to the bare mention of those which have proved most valuable in my own experience.

In cases of chronic rheumatism limited to one or a few joints with considerable effusion, I have used the following with advantage:

℞

Potassii iodidi, ʒ ij.
Hydrargyri bichloridi, gr. j.
Syrup. sarsæ comp., ℥ v.
Ft. sol. S. Teaspoonful in water after meals.

or:

℞

Hydrargyri bichloridi, gr. j.
Inf. gentianæ comp., ℥ vij.
Ft. sol. S. 1 to 2 teaspoonfuls in water after meals three times daily.

In cases where a number of joints are involved with marked tendency to exacerbations, and especially if the lesions of the small joints indicate gouty complications:

℞

Pulv guaiaci, ʒ j.
Vin colchici radicis, ʒ ij. to ʒ iij.
Potassii iodidi, ʒ j.
Pulv acaciæ, q. s.
Sp. lavendulæ comp., ℥ ss.
Aq. cinnamomi, q. s. ad ℥ vj.
Ft. sol. S. Dessertspoonful three times daily in water.

The bicarbonate or the acetate of potash may often be substituted with advantage to the digestion for the iodide of potassium in the above mixture. I have already alluded to the use of prolonged courses of lithia as being very beneficial, especially in cases with a gouty element and with defective action of the kidneys. In regard to the mode of its administration, I much prefer the effervescing granulated salts.

I must also mention the benefit I have derived from the prolonged use of carefully increased doses of Donovan's solution. It is to be remembered that these alteratives have, for the most part, been given while the patient was also taking iron in large doses, cod liver oil, syr. hypophos. comp., or some similar nutrient.

I will merely mention again the nitrate of silver as an alterative, from which I think I have obtained good results, especially in cases attended with neuritis and with marked nervous symptoms.

A SERIES OF CASES OF PARÆSTHESIA, MAINLY OF THE HANDS, OF PERIODICAL RECURRENCE, AND POSSIBLY OF VASO-MOTOR ORIGIN.*

By JAMES J. PUTNAM, M.D.,

LECTURER ON DISEASES OF THE NERVOUS SYSTEM, MEDICAL SCHOOL, HARVARD UNIVERSITY.

WITHIN the past few years there has come to my notice, mainly at the Out-Patient Department of the Massachusetts General Hospital, a large number of cases such as I do not remember to have seen described anywhere in detail, though they are often the subject of brief reference. While differing from each other in minor respects, these cases agree in presenting as a common symptom a disturbance of the subjective sensibility of the skin, giving rise to what is known popularly as numbness, recurring periodically, coming on especially at night or very early in the morning, and affecting one or both hands, either alone or in company with the arms, the legs, or rarely the whole body. This numbness was very often excessively intense, so as to amount to real pain in itself, and sometimes also it was associated with pain of a more or less typical neuralgic character, especially in the arms. Occasionally the numbness was associated also with a sort of rigidity of the fingers involving some loss of power, and sometimes a change of color of the skin of the nature of paleness or red-

*Read before the American Neurological Association, in June, 1880.

ness. In some cases, simply letting the arm hang out of bed or shaking it about for some moments would drive the numbness away; in others, this could only be done by prolonged rubbing and use of the hands in ordinary employments. Under this latter influence it rarely failed to yield temporarily. Most of the patients were women in middle life, and many of them were debilitated or anæmic. On the other hand, a few well-marked cases were in strong, healthy men, one of them being a gentleman-farmer, a vigorous man living a temperate out-of-door life.

In the course of this brief sketch, the criticism will, no doubt, have arisen in the minds of some of my readers that these cases ought not to be made into a nosological group by themselves, a large part of them belonging in the same general class with the neuralgias, and others with the vaso-motor affections, fully described by Raynaud, Vulpian and others, under the name of local asphyxia of the fingers. As a matter of fact, all the symptoms which I have mentioned are met with not unfrequently in cases of well-marked neuralgia, as well as in those of the vascular spasms of the fingers alluded to.

To me, however, one interesting feature of my cases lies in the fact, that while resembling both these affections, as well as another to be mentioned later, they differ from them in well-defined respects, and thus help us to split these groups of symptoms up into their constituent parts, from which the clinical necessity of giving names, which must often be inadequate, is apt to distract our attention. Thus, the term, neuralgia, has practically usurped for the one symptom, pain, the field which scientifically belongs, with almost equal rights, to the trophic and vaso-motor and other symptoms with which the pain is usually accompanied; and it is very instructive to study this analogous series of cases in which the symptoms of so-called numbness, usually

of little account, is brought into the foreground. The same might be said with regard to the affection characterized by vascular spasm of the fingers, between which and one division at least of the neuralgias, the cases to be reported perhaps form a transitional group. The neuralgias to which I refer are those in which the pain is supposed to be due to vascular spasm in the peripheral distribution of the affected nerves, depriving the nerve terminations of their arterial blood supply.

Such is believed to be the origin of the intense pain of lead colic, and, perhaps, of other visceral neuralgias and of migraine, and to it is probably due, in part, the numb, aching pain from exposure of the hands and feet to cold. The pain in the later stages of frost-bites is, no doubt, due on the other hand to congestion of the nerve filaments, and the chances are that this influence, too, plays a part in the cases before us.

Dr. Weir Mitchell* has dedicated a lecture to the symptom, numbness, which covers a good deal of the ground here traversed; but the literature of the subject is otherwise scanty. Most important for our present discussion are the papers by Raynaud (De l'asphyxie locale. Thèse de Paris, 1862; and art., Gangrène symétrique, in the Nouveau Dict. de Méd. et de Chir. Prat.); and Vulpian, (Lecons sur l'appareil vaso-moteur, Paris, 1875.)

I will first sketch the history of one or two illustrative examples, taken nearly at random from my case-book, and then give a brief analysis of the whole number which I have seen.

Case 1.—A married woman, 50 years of age, always strong and well. As a young girl, used to have neuralgia in the face; never since. For the past four weeks she has suffered from intense sense of numbness of one or both hands, from wrists down. It is present to a greater or less degree nearly all the time, but is worse

* *Phila. Med. & Surg. Reporter*, Dec. 2, 1876.

at night, especially from about 3 A. M. till daylight, and also when, after doing housework, she sits down to sew. At such times she finds, after a few minutes, that she can scarcely hold a needle; the numbness, subjective and objective, is so intense, especially on the palmar surfaces of the thumb and fingers. This sensation is often associated with pretty severe pain, felt mostly in the back of the hands; but not with change of color of the hands or with swelling. All the time of her visit to the M. G. H. the numbness was only present in the thumb, and the sensibility to the æsthesiometer was not impaired. The treatment (cannabis indica) has been used too short a time to justify conclusions as to its efficiency.

CASE 2.—Single woman, 20 years of age. Anæmia. Numbness of entire left hand, except thumb; of 3 years' duration, often associated with paleness of fingers; skin of hands hard and dry. (A condition similar to this is described by Raynaud, as present in a few of his cases, and is said to have disappeared with the permanent disappearance of the local asphyxia.)

CASE 3.—Married woman, 24 years of age. Moderate degree of anæmia and debility. Numbness and prickling of entire right hand and arm, associated with deep-seated aching; worst at night and early in the morning; increased also by putting hand in hot water, and by lying down even in the daytime; relieved by leting hand hang down or by rubbing. Hands said to be sometimes warm, sometimes as cold as lumps of ice, and of bluish color; but numbness not worse during either one of these stages than during the other. Finger tips often turn white, especially when hand is used. Tingling easily excited by pressure over various parts of arms, including nerve trunks. Strychnia, phosphorus, galvanism, used successively (the galvanism three times a week for 2 to 3 weeks) without marked results, though the last, applied from spinal cord to brachial plexus, gives sometimes temporary, and possibly, at last, slight permanent relief. When the numbness is severe the fingers feel stiff and helpless.

CASE 4.—Married woman, 40 years of age. Marked anæmia and debility. Numbness of hands, sometimes very intense, and associated with sense of powerlessness of fingers, coming on early in the morning. It has been recurring for 21 years, though much less severe when she has lain in bed for some months at a time, as she has done occasionally from sheer prostration.

CASE 5.—Married woman, 37 years of age. Good health: now in fifth month of pregnancy. For past two months she has suffered from intense sense of prickling in both hands and arms below

elbow, especially in right; worse at night or at 2 to 3 o'clock in the morning, though rarely entirely absent; sometimes associated with aching of hands. During a previous pregnancy she was similarly troubled for about one week. These symptoms are made worse by hard work, and by bad weather. The sensibility of the finger tips of both hands is greatly diminished at times. Fingers are generally warmer than normal, and feel better when cool, though contact with cold water increases the symptoms. The fingers never become pale, but sometimes red, hot, and throbbing, and in the morning they sometimes tremble and are powerless, so that she can with difficulty extend them. Potassium bromide at night gives some relief. Ice was applied to back of neck for twenty minutes on three successive nights without avail. There was finally gradual improvement under constant use of potassium bromide in very large doses, though for a time the symptoms became worse.

CASE 6.—Married woman, 24 years old. For many years she has been troubled with sense of prickling, numbness, and weakness in hands, to such a degree that she sometimes drops what she is holding. It is at its worst early in the morning, though it does not wake her up. Symptoms are increased by contact of hand with hot water; relieved by cold.

Patient reports that her mother was troubled with these same symptoms during pregnancy with patient herself, to such a degree that she could scarcely use her arms. Patient herself was not worse during a pregnancy four years ago.

CASE 7.—Single woman, 22 years of age. Health always good, except that she has backache at times. Lives in the country, and does easy work. For past year she has suffered from numbness and tingling, sometimes quite intense, in left hand, especially finger-tips; worse at night, but felt also in the daytime when she lies down or sits down to do fine work. Some relief from letting hand hang out of bed and from rubbing. Usually the sensation is that of prickling, but at times there is intense pain, as if fingers were swollen and tightly compressed. They are, however, not swollen. When symptoms are present, hand feels cold and moist, but does not change color. Great and permanent improvement and final cure under use of phosphorus (gr. $\frac{1}{24}$ t. d.).

To these cases I will add but one more, to show that neither the male sex nor vigorous persons are entirely exempt from this affection.

Case 8.—Patient is a man, 40 years old, and lives an out-of-door, temperate life, in charge of his own farm in the country. Smokes, and drinks tea and coffee, but moderately, and for a long time took neither of the two last-mentioned to see if symptoms would be relieved, which they were not. He is free from other symptoms than those now under consideration, except that he has noticed that his legs "go to sleep" unusually early (that is, when in constrained position). He has been annoyed for several years by numbness of one or both hands, especially left, coming on at about six in the morning, and passing away when he gets up or lets the hands hang out of bed. This condition is worse in winter than in summer. It is not associated with any change in the appearance of the hands. Much relief while taking pills containing strychnine.

Of good cases, answering pretty nearly to the descriptions given above, I have notes of 31,* besides several neuralgias, in which numbness was a marked though not the most prominent symptom, and of two cases of local asphyxia. I am not prepared to say, however, that none of these 31 would by others be called cases of neuralgia, nor to deny, on the other hand, that they may be found capable of further subdivisions. Numbness is an indefinite term, and may at times have been used simply to describe a dull aching neuralgic pain without marked perversions of subjective sensibility, such as prickling, formication, sense of heat and cold, etc. To the majority this criticism would not, however, apply.

Of these 31 patients, 28 were women, three men. Of the women, 21 were married and three single. Of four, no record is made in regard to this point. Almost all the patients were in middle life. Thus there were :

None	below	20	years	
12	between	20	and	30
13	"	30	"	40
6	"	40	"	50
6	over	50		

* Since this paper was first prepared, I have seen a number of new cases.

Ten of the cases were noted as being in poor health; five in good health. Tea and tobacco were a possible cause in one rather anomalous case in a male patient; pregnancy in two or three cases, and in one reported to me by another patient. Cases of recognized organic disease of the nerve centres were, it need hardly be said, excluded. The symptoms were confined to one side of the body (hand, or hand and arm, or hand and leg) in fifteen cases; in fifteen others they involved both sides. In one case the whole body was thus invaded.

In the large majority of cases one hand was worse than the other, however, and, as a rule, certain fingers were more severely affected than the rest; oftenest those supplied by the median nerve, though not necessarily all of these at one time. With regard to the duration of these symptoms, they might be said to have been chronic in almost every case; always lasting for weeks, usually for months or years. Of interesting accessory symptoms coldness and pallor of the fingers is especially worthy of mention. This is noted in but two or three cases, and in them it recurred periodically with the numbness. A larger number of patients said their hands felt full and swollen; actual swelling was, however, objectively confirmed by myself in only one or two cases. I rarely saw the patients, however, when the symptoms were at their height. In two cases the skin of the palm of the hand was thick and dry, indicating a change which may have been due to the patient's occupation, though no sufficient cause of this kind could be traced: it is worthy of note in connection with a similar change which occurred in some of Raynaud's patients. Several of the women spoke strongly of the effect of water—sometimes cold, sometimes hot—in either aggravating their symptoms or in causing an indescribably unpleasant sensation; cold weather, also, almost always increased the symptoms, and they were worse

in winter than in summer, In three, a similar exacerbation followed leaning upon the arms in scrubbing floors or at other work, and others complained that the arm or leg "went to sleep" with unaccustomed readiness when placed in constrained positions. This same increased liability to "go to sleep," affecting the legs of patients with chronic myelitis, has been commented on by several writers and has struck me most forcibly on several occasions.

The differential clinical diagnosis has to deal mainly with *neuralgia*, by which is meant the typical neuralgia, such as is generally believed to be of central origin, with *vascular spasm* of the fingers, the "local asphyxia" of Raynaud, and with *neuritis*.

From neuralgia, the affection before us is distinguished by the following facts:

1. Both sides of the body are so often affected at once (in one half of my cases).

2. The course of the affection is so chronic, lasting often without remission for months and years, and yet not necessarily associated with depressed health, and not increasing in violence as time goes on.

3. The sense of "numbness" or prickling is so strongly marked in proportion to the pain.

4. The pain, when present, is usually of a dull aching character, and often apparently rather an intensification of the "numbness" than acute pain of any familiar kind.

5. Even when the pain is severe it rarely involves the whole of the distribution of the brachial plexus, or is confined to the distribution of any one nerve, often involving only the hands.

6. A high degree of local anæsthesia is often present. Though these signs would have to exclude a typical central neuralgia they might not exclude all neuralgias of peripheral origin, nor should we expect such to be the case.

Neuritis, unless of a very low type, is excluded by the absence of muscular wasting, or of notable trophic changes in the skin and its appendages.

In one case (Wilson) the arm was said to swell frequently and remained almost constantly warmer than the other. This was not, however, a typical case (*vide infra*).

From the local asphyxia of Raynaud, this affection is distinguished by the following facts:

1. The change of color indicative of vascular spasm is much less prominent than in the local asphyxia, while the "numbness" is more so. In many cases no change of color was present, even when the pain was intense.

2. By its more distinctly marked periodicity, the exacerbations being almost invariably in the night, especially toward morning, whereas, as a rule, the local asphyxia is brought on by exposure of the skin to cold or cool air. Still, a type of the local asphyxia in which the symptoms recur regularly and spontaneously, is recognized by Raynaud; and, as in them, certain fingers, and always the same ones, are usually the first victims.

On the other hand there are several points of relationship between the affection described by Raynaud and the one now before us. The pain in both is of much the same character. It is thus described by the French writer: "A depressing sense of numbness and tingling is first felt, and this is followed by sensations of burning and stinging, which pressure intensifies. At the same time the tactile sense of the skin may be so much impaired that it is difficult for the fingers to retain small objects in their grasp."

During the period of reaction, while the small vessels are dilating, the parts are said to be the seat of sensations of intense distress like that caused by frost-bites.

Similar descriptions were very often given by my pa-

tients, though it was, perhaps, more common for them to speak of "aching" pains, and, as has been said, these are sometimes sharp and darting, and may occupy the whole arm, as indeed is true also of the cases described by Raynaud.

In both diseases the symptoms are worse in winter than in summer; in cold and stormy than in warm weather.

Raynaud says: "It is not uncommon to find a woman complaining of this premonitory numbness off and on during two or three years, the symptoms passing away with the coming of summer and returning with the winter, and ending eventually in symmetrical gangrene."

None of my cases have terminated in this way to my knowledge, though I have histories of some of them extending over many years. On the other hand two patients, if not more, have distinctly spoken of the disappearance of the symptoms during summer. In both affections, women, usually debilitated but sometimes in perfect health, are by far more often attacked than men, and the middle years of life (20–30–40) show the largest number of cases.

In neither affection can any special diathesis (rheumatic, gouty, etc.) be assumed as a cause, nor, in my cases at least, had the patients been especially subject to neuralgias. On the whole, especially when we reflect that in some of my cases also there was a marked change of color of the affected parts, it will seem that the resemblances between these two affections are quite striking enough to suggest the possibility of a relationship.

In view of this fact I venture to suggest as a possible cause for the symptoms in the typical cases of the affection here described, alterations of the blood-supply of the smaller branches or terminal filaments of the sensitive nerves supplying the affected districts, occurring, as a rule,

independently of any notable changes in the general cutaneous circulation of the part.

I am well aware of the difficulties in the way of establishing this opinion, and of the impropriety of asserting it dogmatically. So far as the evidence to be drawn simply from the character of the prominent symptom itself is concerned, "numbness" is of course to be met with as a result of the excitation of any part of the sensitive tract, from the cortex cerebri to the peripheral expansion of the cutaneous nerves. The distribution of the symptom is a better guide to the localization of the disturbance.

Thus, the fact that, in our cases, both hands are usually involved, renders a cerebral lesion improbable, and similarly, the non-coincidence of the area over which the symptom is felt with the distribution of any one nerve, renders it somewhat improbable, though certainly not impossible, that the lesion is one affecting the principal nerve trunks of the brachial plexus.

The cervical enlargement of the spinal cord might be the part primarily concerned, but in our entire ignorance of the arrangement of the nuclei of the sensitive nerves in the cord, it would be impossible to assert such a view with confidence.

In favor of the suggestion which I have advanced—purposely in a somewhat vague form—are: The analogy of these cases, with the local asphyxia of Raynaud (in some of the cases there was definite change of color in the skin), and the fact that this theory would reasonably explain the phenomena under consideration, inasmuch as it has been established beyond much doubt (Waller, Mitchell, Reynaud, Vulpian, Richet, etc.) that alterations of the circulation in nerve trunks, whether of the nature of anæmia or hyperæmia, may give rise to the whole range of paræsthesiæ, and to pain.

Finally, whether the hypothesis is correct or not, or

whether it is valid for a certain number of cases and not for the rest, it is one which may fairly demand investigation. If it should prove tenable, the numbness in these cases would be analogous to the pain in the so-called neuralgias of vaso-motor origin, such as lead colic and migraine.

To accept it we should have to admit that the vessels of circulation in the nerve branches have a regulatory apparatus more or less independent of that for the vessels of the overlying skin. I know of no data for establishing this assumption except, possibly, the fact that the hand, or other part, may feel very cold, although the surface is abundantly supplied with arterial blood.*

On the other hand, the autonomous conditions of the circulation in the muscles and viscera furnish sufficient analogy for such a state of things.

Perhaps a stronger argument against the hypothesis is the fact that those symptoms of numbness, etc., may occur not only confined to a definite part, like the hand, but spread over a large and ill-defined region, as the legs in Berger's cases (*vide infra*), or even the whole body. It is hard to say just what significance is to be attributed to this fact.

If the question be put as to the ultimate cause of the vaso-motor spasm, I should refer to the discussion of this question by Raynaud and Vulpian, apropos of the local asphyxia, the former holding that it is due to an abnormal state of the spinal vaso-motor centres, the latter pointing out that the action of the local muscular mechanism may likewise play its part.

In the foregoing descriptions of my cases, one or two special symptoms were mentioned which deserve a moment's attention. Thus certain patients were quoted as saying that to plunge the hands into hot or cold water caused very unpleasant sensations. This, or a kindred phenomenon, the increased sensitiveness to pressure met with

* *v.* Runge. *Arch. für Klin. Med.*, 1870.

in the local asphyxia, is explained by Richet under the supposition that the peripheral nerves are made more excitable by the loss or diminution of their blood-supply. In order that this phenomenon may be present the hyperæsthetic portion of the nerve need not be its periphery, but might be at any part of its course or at its centre (*v.* Eulenburg, *Leitungsneuralgie*); and indeed this same increased sensitiveness to hot and cold water is occasionally seen in cases of meningitis, as from Pott's disease. Still it is by no means a common event, and must be rarely seen in true neuralgia even of a severe type, and I look on it as probably a real sign of local irritation of the nerve filaments in the hand itself, in the cases now before us.

Why is it that night and early morning are so regularly the time for these morbid phenomena to make their appearance? It can hardly be alone because, his business done, and the house quiet, the patient is at liberty to think of his discomforts, for the pain often wakes him from a sound sleep, during which his sufferings had presumably been forgotten. It is at this same hour, 3 or 4 A.M., that epileptic attacks are so apt to occur, and that death reaps his largest harvest. The exacerbations of pain of some forms of neuralgia, and especially that of neuritis, are also apt to occur at this time, as is well known, and this consideration, taken together with the fact that in at least one of my cases (*vide infra*, John Wilson), there were distinct signs of neuritis of a very low type, furnishes additional evidence that the characteristic lesion is one of the peripheral nerves and not of the nerve centres.

With regard to the treatment of these cases, I have had too little opportunity to test the different modes carefully to make a statistical statement of any real significance, and I can only offer my experience as of suggestive value. I have used galvanism, phosphorus, strychnia, bromide of

potassium, cannabis indica, nitrite of amyl, and a few other remedies. One of the male cases, of years' standing, improved quite remarkably while taking strychnine, which had been preceded by phosphorus, and in several others, phosphorus certainly seemed to be of great efficacy,—greater than any of the other drugs.

Nitrite of amyl I should like to try further before speaking of it.

Galvanism was tried faithfully in a number of cases, the current being passed along the arm in various fashions, but though it seemed to help from time to time, I was not impressed in its favor on the whole. Raynaud has reported recently several cases in which he obtained excellent and permanent results from galvanism of the upper part of the spinal cord daily, for a number of weeks, and I should be encouraged thereby to try again. Unfortunately, an Out-Patient Department is not the best place in the world for making careful therapeutic investigations.

It is not to be supposed that, with the description of the affection which has formed the subject of this paper, I regard all the cases or groups in which "numbness" or "paræsthesia" is the most prominent symptom, to be disposed of. One or two cases are in fact included in my list, which perhaps deserve to be classified separately, where this symptom instead of recurring periodically remained constantly present, whether seated in the hands,—or rather fingers,—or in the feet and legs. One case of this kind I will refer to, as I have learned some new points in his history within the past few days.

John Wilson, a middle aged man of temperate habits and good health, a currier by trade, and obliged, in working, to bear with force against a wooden scraper held in both hands, has suffered for the past fifteen years from a sense of "numbness" and prickling in the left hand and arm below elbow.

This sensation rarely, if ever, leaves him entirely, night or day,

but is intensified by cold weather and by hard work, and is very bad at night, often waking him up from a sound sleep. When in its mildest form, it is felt only in the fingers ; when worse, in the hand, and finally in the arm, as stated ; but it is at no time confined to the distribution of any single nerve. At times the feeling is so intense that it amounts to severe pain, but this is of an aching character, and is distinctly recognized as being only an intensification of the paræsthesia, not something apart from this. It is described as being like that felt when one's leg is severely "asleep," or something like the benumbing pain of cold. The hand does not change color. A moderate degree of the prickling is not incompatible with (apparently) perfect tactile sensibility, but when it is severe the sensibility is said to be much impaired. The muscular power of the arm is as good as ever when it is not "numb," but then the fingers and arm feel stiff and helpless. Shaking the arm in the air gives relief for a while. At times he thinks the arm swells, and at one visit to my office the left arm was distinctly warmer than the right, even though, as it was Sunday, the numbness was not at the time severe.

Dr. S. L. Abbot has related to me a similar case, in which the arm felt sore on pressure. In that case, salicylic acid in doses of grs. x, seemed to give great relief.

One would be led by the history of these cases to suspect neuritis or pressure on the nerve trunks, perhaps, in the former, by the muscles themselves, as sometimes happens, but this would seem to be excluded by the fact that the distribution of the sensation is not coextensive with that of any one nerve, and that it is felt as well at night as during use of the limb. Still, I do not regard the case as typical of the group of which I have been mainly speaking.

The nerves of certain persons, even in perfect health, seem to be more susceptible to the effect of pressure than others, and more under certain circumstances than under others. Thus I have noticed with myself that the pressure of a heavy coat or of the strap of a knapsack, while it is liable to make both arms "go to sleep," affects the left arm far sooner than the right, and certain fingers (the forefinger

and thumb) before any of the others, and that this happens much more readily on a hot, oppressive, than on a cool, day.

An analogous group of cases to those reported in this paper (based on six observations) has recently been described by Dr. O. Berger of Breslau (*Breslauer Aertzl. Zeitschr.*, 1879, Nos. 7 and 8), and given in abstract in the *Centralblatt für Nervenheilkunde*, etc., 1879, No. 12, by Möbius. The main symptom is described as attacks of intense paroxysmal and very unpleasant, though not sharply painful, paræsthesia (formication, prickling, "numbness," burning) of the *legs*. These attacks occurred after protracted sitting or standing, not after walking. The sensation either began to be felt in the neighborhood of the hip-joint and spread to the foot, or *vice versa*. In some cases the arms were involved, and very often one arm and one leg were preëminently or exclusively attacked. The disturbance of sensibility was always associated with a sense of great muscular helplessness, interfering with voluntary motion. Each attack was of but a few moments' duration, but they recurred at short intervals. The aggregate duration of the affection was chronic up to nine years, as in many cases, and the general health of the patients was usually good. Of the six patients, four were men, two women; all were in middle life, and of the well-to-do classes of society.

Berger believes the affections to be of spinal origin, and found nothing, except arsenic, of value in the treatment, and that only as a palliative. No argument will be necessary to show the analogy between these cases and mine, though it would be premature to claim that the pathology of both must be the same, or even that it is identical in the whole number of my own cases. Taking all together, it will be seen that there are several, perhaps quite a number of more or less distinct types, and that in one form or another they are far from rare.

III.

ON THE USE OF THE COLD PACK FOLLOWED BY MASSAGE IN THE TREATMENT OF ANÆMIA.

By MARY PUTNAM JACOBI, M.D., and VICTORIA A. WHITE, M.D.*

IN the August number of the ARCHIVES, we began to analyze the therapeutic results obtained by us in the cold pack, in the light of some of the most recent researches on the physiology of nutrition. We directed particular attention to the fact that, in anæmia, the same condition of the muscles which renders voluntary exercise difficult or impossible, exists to interfere with the benefits that might be expected from massage.

Since in health, and after adequate repose, voluntary muscular contraction determines an increased flow of blood to the contracting muscles, it is often expected that passive contraction of anæmiated muscles will do the same thing, and thus obtain at once, increased nutrition of the muscles, and derivation of blood from the internal organs. But we have seen that experiments upon *fatigued* muscles show, that the contractions which may be excited in them are not effected in precisely the same way as in health. The blood-supply does *not* increase indefinitely in proportion to muscular exertion; indeed, if it did, muscular fatigue would be impossible, and the muscle could go on contracting forever. But after a certain limit of exertion has been reached, although increased stimulus will continue to cause contrac-

* Continued from Vol. iv, No. 1, p. 72; and concluded.

tions at the expense of the material stored up in the muscle, new material ceases to arrive or be assimilated, and the acid fatigue-products accumulate because insufficiently washed away. There is thus double proof that the blood stream is lessened, and that the muscular contractions then performed do not suffice to increase it.

Anæmic muscles resemble fatigued muscles in that their blood-supply is insufficient, and their contraction is therefore attended by pain. They differ, in that in anæmia this state has not necessarily been determined by excessive muscular exertion. But from this difference we must not, therefore, infer that the insufficiency of blood-supply is the primary element of the morbid state: rather that this has been determined, in many cases at least, by the inaptitude of the muscle to appropriate to itself material from the blood out of which to construct its contractile material in sufficient abundance. This inaptitude, often congenital, may certainly be closely compared with that which is induced when, by prolonged contraction, the store of contractile material in the muscle is nearly exhausted, and, when contraction, during the persistence of the muscle, is unable to renew the supply. *Why* it should be so unable, I believe, is not at present clearly understood, but the fact, remarkable as it is, is demonstrated both by exact experiment and by common experience.

Under these circumstances, if we wish to enlarge the blood-supply and increase the nutrition of the muscles, we must reverse the initial procedure, and, keeping the muscle at rest, endeavor to attract blood to it by methods acting directly on the blood-vessels.

We have seen that friction, as employed in methods of massage, is one means of dilating the blood-vessels. It is a method synergistic with that initial effect of the cold pack already described ;* whereby, after stimulation of the sensi-

* ARCHIVES, August, 1880, p. 56.

tive nerves of the skin, the influence of the vaso-motor constricting nerves is overcome, and the blood-vessels dilate.* When the cutaneous blood-vessels are dilated, we may expect that those in the superficial layer of muscles will be dilated also. Further, since stimulation of nerves terminating in muscles has been shown by Ludwig to cause dilatation of the blood-vessels of these muscles (see note below); and since the cutaneous nerves, doubly stimulated by cold and by friction, constitute the superficial part of the plexus which sends nerves to the muscles lying deeper, we perceive additional reasons for expecting an afflux of blood to these muscles during the pack and massage. Of the two stimulants we should expect the cold to be more effective in determining this afflux, than the friction, because of the special susceptibility of the heat-producing muscles to thermic irritations. The rapid production of heat observed to follow the application of cold to the surface of the body, can only be effected in virtue of a dilatation of muscular blood-vessels, which, as the result shows, is brought about immediately after the abstraction of heat.

In the cold pack, the production of heat continues until the wet sheet has been warmed through. The warming is facilitated by the blankets which, preventing the escape of heat, utilize all that is produced. During all this time the

* The statement made on page 57 of our second article, that "this active dilatation of cutaneous blood-vessels, is effected under the influence of the cerebro-spinal nerves," expresses the sequence of many easily observed facts, without entering into the still disputed question of the existence of special vaso-motor dilating nerves. In regard to these, Hermann remarks: "The existence of direct vaso-dilator nerves, as maintained by Bernard and Schiff, is still undemonstrated."

We would subscribe to the further remark of this distinguished physiologist: "Their modes of action in any case were incomprehensible."—*Physiol.* p. 74. Berlin, 1874.

The demonstration of vaso-dilating properties in the chorda tympani, the sciatic, and some other nerves, certainly affords no proof that the dilatation is effected by special fibres, rather than by inhibition of the vaso-constrictor nerves. Researches made since the publication of Hermann's treatise, have indicated with probability the existence of local vaso-motor mechanisms, acting differently upon the blood-vessels to which they belonged, according to the different nerve fibres which liberated their activity. Ludwig has shown that stimulation of muscular (spinal) nerves always causes a *dilatation* of the blood-vessels of the muscle.

afflux of blood to the muscles continues, and is only checked when, in the establishment of an equilibrium of temperature, the thermic irritation disappears (see p. 66 of our second article). Thus is secured the necessary preparation for muscular exertion which is so important to obtain where anæmiated muscles are compelled to contract. By it these muscles are brought into a condition of vascularization approximating that of health ; their contraction, therefore, may be followed, as in health, by a still further afflux of blood and by increased chemical metamorphosis,* leading ultimately to a larger accumulation of contractile material.

We may now profitably return to the main purpose of this paper, and inquire what relations, if any, exist between the phenomena that have just been described, and the modifications of the urine we have ascertained by experiment to take place.

An increase of urea, after the pack, has been observed by hydropathists in isolated cases, as quoted from Wemott by Lersch † among other writers on the subject. Winternitz mentions an increased secretion of urine as the result both of the cold pack and of friction in the cold wet sheet. He attributes this entirely to the increased arterial pressure in the abdominal vessels, caused by the contraction of the cutaneous blood-vessels, and relates cases to show that under certain circumstances the rise of tension may be sufficient to cause albuminuria (?). At all events, albuminuria was observed as a consequence of the treatment.

Ludwig's famous experiments upon the effect of increased or diminished pressure in the renal artery sufficiently de-

* We do not say, as is sometimes said, "increased *nutritive* metamorphosis ;" for it seems conclusively proved that for the muscle, as for the nervous tissue, the period of nutrition does not coincide with the period of functional activity, but alternates with it. Thus, as has been so remarkably demonstrated by the respiration researches of Pettenkofer and Voit, more carbonic acid is eliminated during the day and during exertion ; but more oxygen is absorbed at night and during repose.

† Loc. cit., p. 247.

monstrate the possibility of increasing at least the excretion of water from the kidneys by means of an increase in the pressure of the abdominal blood-vessels. Neither can there be any doubt that this abdominal pressure is increased during the first period of the pack, when the blood is driven from the surface of the body. It cannot do otherwise than accumulate in the vast vascular area of the abdominal blood-vessels, and the mechanism by which this is accomplished is pretty well known. When the blood-vessels of the periphery contract, pressure on the central parts of the vascular system increases. Foremost among these central parts is the centre of all, constituted by the cavities of the heart: increased pressure here stimulates the depressor nerve; this stimulus conveyed to the medulla inhibits the vaso-motor centre, and especially the vaso-motor nerves which run to the abdominal blood-vessels in the splanchnics. Hence the dilatation of the abdominal blood-vessels which takes place, and in proportion to the contraction of the cutaneous.

But if the above series of events were the only cause of the diuresis observed after the pack, its influence should terminate as soon as the cutaneous blood-vessels begin to dilate. As much urine should be secreted after a pack of fifteen minutes' duration as after one of two hours. This, however, was by no means the case. With some persons, indeed, a sense of fulness in the bladder made itself felt early in the pack; but this was rarely very noticeable under half an hour. In an hour it generally became very marked, and if the pack lasted two hours was liable to be almost unendurable. In some cases not included in our list, and in which the urine was not analyzed, the patient was always compelled to empty the bladder immediately upon being taken from the pack, and before submitting to the massage.

It would seem, therefore, that the increased elimination

of urine, begun during the first period of the pack, continued throughout it, either under the continued influence of the original cause, or of some other condition, coincident or supperadded.

Now, during the grand oscillatory movement of the circulation we have now so often described, all the tissues of the body are necessarily subjected to a more abundant osmotic "streaming." It is characteristic of anæmic tissues that they habitually retain a much higher degree of water than is normal (Ranke). In many cases of the so-called "fat anæmia," the tissues—especially the nervo-muscular tissues—are probably "waterlogged," or have become nearly as hydræmic as the blood. It is extremely probable, though difficult to demonstrate experimentally on the human subject, that a considerable portion of the water eliminated by the kidneys during the pack, is derived, not from the blood alone, but from the tissues. If at any given time exosmosis from the tissues has been arrested by an abnormal equilibrium of density between them and the blood, removal of a certain amount of water from the blood should, as in the eliminative treatment of anasarca, by raising the density of the blood, facilitate diffusion into it of liquid from the more watery tissues. Such diffusion, continuing during the second period of the pack, would continue to increase the amount of water passing to the Malpighian tufts of the kidneys.*

But, in the third place, the question presents itself whether water be not formed in the organism during the pack, as Flint suggests that it is formed during excessive muscular exertion. That more water may be eliminated from the body than has been ingested, is unquestionable. Thus, in one of Voit's hunger researches, where the dog re-

* In nervous tissues the percentage of water is normally greater than that of the blood, but the habitual excess is somewhat increased during exhaustion (Ranke, Tetanus). It is this increase which would be removed by a diuretic agency.

ceived only 33 grms. of water and 358.1 grms. of oxygen in the day, he eliminated with the urine 105.6 grms., and with respiration 400.5 grms., or a total of 506.1 grms. of water.*

In this experiment, the disproportion between the amount of water ingested and that eliminated is much greater than is generally seen in ordinary conditions of nutrition. Indeed, as Funke remarks,† the disproportion is more apt to be the other way; only one-third the amount of water which has been ingested is eliminated, the rest is stored up in the body, increasing its weight. But in the conditions of the experiment the body was wasting; with especial rapidity was fat disappearing. As far as we now know, the complete disappearance of fat from the body implies its ultimate conversion into carbonic acid and water, with increased elimination of the latter both by lungs and kidneys.

Now, during the increased production of heat artificially determined by the cold pack, as during the increased production of fever (Liebermeister), there is an increased elimination of carbonic acid,‡ to be attributed to increased oxidation of non-nitrogenous, *i. e.*, carbo-hydrate substances contained in the muscles. The oxidation of carbo-hydrates is not as necessarily attended with the formation of water as is the oxidation of fat; but it may occur,§ and, in view of the increased elimination of water demonstrated to coincide with the increased elimination of urea, we may infer that it does.

The elimination of an excess of water from anæmic tissues, cannot fail to be an immense advantage. "Fat an-

* Pettenkofer and Voit. *Zeitschrift für Biol.*, Bd. v.

† *Lehrbuch der Physiologie*, Bd. i. Voit claims to be the first to call attention to the great variation in the amount of water contained in the body of the same individual at different times. But it is Ranke who has estimated this minutely for isolated muscles and nerves.

‡ The large amount of water eliminated in some of the wasting diseases of children, especially during the incipiency of tuberculosis in the brain or elsewhere, may *possibly have a* similar origin.

§ Winternitz. Loc. cit., Bd. ii, p. 391.

æmias," proverbially difficult of cure, may benefit greatly by the elimination of water effected in another way, namely, by the Turkish bath. But this bath has, for many patients, many inconveniences, and can by no means take the place of the pack.

The increased circulation of water through the abdomen, and hence through the chylopoietic viscera, is of great importance in improving the nutrition of anæmics. We have already dwelt upon the manner in which the pack may act beneficially upon the chronic gastric or intestinal catarrhs so frequent in anæmic persons. But there are many cases in which such catarrh does not exist; cases in which, however, nutritive absorption seems to be slackened, and, perhaps, the elaboration of the digestive products in the chylopoietic viscera imperfectly performed. In these cases, although no other symptom of primary indigestion exists, there is very generally constipation. We may infer that the torpor of the muscular coat of the intestine extends to the muscular fibres of the villi, whose central duct thus fails to receive the peristaltic compressions which favor the first onward movement of the chyle. Of all the causes which should retard the elaboration of digestive products in the liver and other glands, many are imperfectly known to us; but a languid circulation may certainly be counted as one. Increased force of circulation of the liver, as effected by the pack, produces the same results as the ingestion of large quantities of water; the entire portal circulation, both vascular and lymphatic, is stimulated; more material is taken up from the alimentary canal, the metamorphoses of this are facilitated, and, as an ultimate result, the production of urea increased. This increased production of urea is often still accepted as proof of increased nutritive assimilation in the tissues; although, in itself, an increased elimination of urea can only indicate an increase of

waste or disassimilation. But at present we are not authorized, without special proofs, to derive the urea from the organized tissues at all.

The famous researches of Bidder and Schmidt, those of Bischoff and Voit, and especially the numerous and more recent researches of the latter, prosecuted alone or in association with Pettenkofer, have brought into view an entirely new set of conceptions in regard to the nutritive metamorphosis of albumen. According to these views, the urea eliminated in ordinary normal conditions is not derived from albumen that has become organized into tissues, but comes from the albumen of the food yet retained in the circulation. The destructive metamorphosis of this albumen, that never is used for plastic purposes, constitutes what Schmidt first called the "Luxus-consumption." To it should be largely due the elaboration of force employed in the various mechanisms of the body.

The proof of this doctrine is found in the following facts of observation and experiment:

It has long been a matter of observation that the amount of urea is more precisely and markedly affected by the nature of the food ingested than by any other influence; an increase of the albumen of the food is immediately followed by a corresponding increase in the urea of the urine; and a diminution of albuminous food will, with similar promptness, cause a diminution of urea.

On the other hand, as has been more recently demonstrated, it is certain that the increased functional activity of richly albuminous organs—the muscles—does not increase the amount of urea in the urine. It would be superfluous in this place to even allude to the well-known experiments by means of which this demonstration has been effected.* But it is, perhaps, not superfluous to indicate the line of

* See especially Fick and Wislicenus; also Parkes already quoted.

proof for Voit's now celebrated doctrine of "circulating," "organized," and "store" albumen, inasmuch as this doctrine is not yet universally admitted, and the questions involved in it are important in the pathology of anæmia.

Voit* estimated the urea excreted in 24 hours by a healthy dog, then deprived the animal of all food during a number of days. On the first day of starvation of a dog previously well nourished, there was no marked difference in the amount of urea; but on the second day was noticed an abrupt fall in the amount of urea eliminated, and this amount continued to diminish for several days. A period of equilibrium was then reached, in which the daily elimination of urea or of nitrogen remained sensibly the same. Since no food was taken, and since time enough had elapsed to consume all remains of food stored up in the organism (the period of this consumption being indicated by the progressive variations in the eliminations of urea), the urea now eliminated could have no other source than the albuminous tissues of the organism. From the nitrogen of this urea, which constituted 3.4 per cent. of the fresh albuminous tissues, the amount of these that had been decomposed in the process of wasting, could easily be calculated. This amount, however, did not account for the entire loss of weight, as observed from day to day. By means of Pettenkofer's respiration apparatus, the loss of carbon, hydrogen and oxygen, in excess of what could have been contained in the albuminous tissues and eliminated from the body as carbonic acid and water, could also be ascertained, and the sum total of losses thus estimated was found to correspond to the total loss of weight sustained by the animal during the period observed.

* Ueber den Kochsalz; also many essays in *Zeitschrift für Biolog.*, from Bd 1 onward. See also analogous researches of Adamkiewicz, Das Pepton, and criticism (favorable to the theory) of Funke, *Lehrbuch der Physiologie;* Landois and Hermann, in their respective text-books, accept Voit's doctrine without reserve.

The amount of nitrogen eliminated during the "stationary" period, or while, with complete inanition, this amount remained constant from day to day, was accepted as measuring the proportion of organized albumen capable of oxidation and of conversion into urea. In later periods of inanition,—when the animal lived so long,—the amount of urea increased, and, coincidently, starvation fever set in; double indication of an abnormal destruction of albuminous tissue. During the earlier period, on the contrary, the larger amount of urea must have been derived from food previously taken. For it varied in proportion to the albuminous richness of this food; and the variations were much greater than could be explained either by the supposition that urea had previously been retained in the body, or by presumed variations in the condition of the albuminous tissues.

By subtracting from the urea of the first hunger day that of the stationary hunger days, was obtained the proportion of urea which is derived from the stored-up albumen.

Thus, in one of Voit's dogs, the urea on the first hunger day was 37.5 grms. On the fifth day, when the elimination had become stationary, it was 12.6 grms. This, subtracted from 37.5 grms., leaves a residue of 24.9 grms., which must have been derived from the albumen of the food previously ingested. This 24.9 grms. is 66 per cent. of the whole amount of the first day. From a series of similar experiments, Voit arrived at an average of 70 per cent. as belonging to the stored albumen, and used for consumption when the organism was deprived of food.

When, to a starving dog arrived at the stationary period, food was given in increasing quantities, an ascending series of urea eliminations was constructed, wherein the excess of urea corresponded to the amount of nitrogen contained in

the food ingested. Nor did only the excess so correspond; for, with increasing quantities of nitrogenous food, a condition was reached in which all the nitrogen of both urine and fæces was covered by the nitrogen ingested; and there was no surplus to be accounted for by decomposition of organized tissues. This is the condition of Nitrogenous Equilibrium,—when the food entirely protects the tissues from oxidation, and when the body, though eliminating immense quantities of urea, may actually gain in weight.

From these data we cannot see how it is possible to avoid the conclusion that, in all circumstances but those of starvation, the greater proportion of the urea of the urine comes from the food ingested on the same day of the observation, or during the two or three days previous. Voit admits, from the result of other experiments and calculations, that in ordinary circumstances, about one per cent. of the daily urea is derived from the organized albumen of the tissues, *i.e.*, from their wear and tear.

To this theory Foster * objects that it involves the assumption that oxidations of albuminous substances habitually take place in the blood circulating in the vessels; and he considers this assumption inadmissible on account of many facts which indicate that oxidations never take place in the blood, but only in the tissues.

It is difficult to see how such an objection can be urged by any one who has seriously studied Voit's elaborate papers on the matter. For his expression, "circulating albumen," by no means implies albumen still contained in the blood-vessels, *but albumen circulating through the elements of the tissues and not organized into their structure.* "It is always the nutritive fluid changing and circulating through the organs, which is decomposed, and not the organised part of the muscles. * * Since the muscles constitute

* Text-book of Physiology, p. 362.

the greatest bulk of the organs, in them must the most albumen be decomposed, and the greatest mass of the nitrogen of the excreta must come from them."—*Zeitschrift für Biol.*, Bd. i, p. 238.

In researches on cholera uræmia, Voit found a large amount of urea in the muscles (where, normally, as is known, exists only a trace). He observed the same thing in nephrotomized dogs.

The great generalization made by Voit in regard to the purpose of nitrogenous metabolism, precludes the idea that he imagined this to occur in the blood-vessels. He * compares the stream of albumen passing through the elements of the tissues, to the stream of water passing over the wheel of a mill. As the mill wheel turns in virtue of force derived from the water, so the functions of living tissues † are performed in virtue of force derived from the constant decomposition of albuminous molecules, and [is implied] as the mill must be immersed in the water, so the tissue-elements must be in immediate contact with the albuminous molecules, whose more or less explosive decomposition is constantly liberating force.

Foster himself offers a suggestion which is by no means incompatible with the theory of Pettenkofer and Voit. It is known that leucin is formed in the intestine during the pancreatic digestion of albumen, as it is in artificial oxidations of the same substance; known also that leucin is convertible into urea, and that unless large quantities have been ingested, experimentally, into the alimentary canal, no leucin, but an excess of urea, appears in the urine. From these facts it may be, perhaps, inferred that the "Luxus-consumption" of large quantities of proteic material is effected in the intestine by conversion of albumen

* Pettenkofer and Voit. Untersuch. über den Stoffverbrauch.—*Zeits. für Biol.*, Bd. ii, p. 368.

† As, for instance, the muscular tissues.

into leucin, the latter, being absorbed, passing off in the urine as urea.

Taken simply in this way, however, the "Luxus-consumption" would have no physiological significance further than as a provision for getting rid of proteid material ingested in excess of the real needs of the body; a mere provision against gluttony. It is inconsistent with many facts, and especially with those indicating that the decomposition in the organism of albuminous substances, as well as of carbohydrates, is necessary for the development of the vital forces. "Indeed," observes Foster himself, "the whole secret of life may almost be said to be wrapped up in the occult properties of certain nitrogen compounds."*

The real theory of the "Luxus-consumption" implies that these metabolic functions constitute the principal uses of albuminous food, and explains, therefore, why an amount of such food, largely in excess of what is needed for the repair of tissue, may be not at all in excess of the total needs of the economy. It implies, moreover, that these needs are met, during the series of changes that takes place in albumnoids, after absorption from the intestine.

Leucin, however, is far from being the only product of pancreatic digestion. So far as we know at present, it is only a minor, an accessory product. Peptone is formed in the intestine as well as in the stomach, and Adamkiewicz's experiments show that peptone may be organized into the tissues, and also may be eliminated in the form of urea. It is thus certain that leucin is not the only antecedent of urea; it is even probable that the latter may be derived from several different substances, each of which represents a preliminary term of albuminoid oxidations.

It is the leucin origin of urea which has furnished a weighty argument in proof of the formation of urea in the

* Loc. cit., p. 371.

liver.† For in acute fatty atrophy and some other diseases of the liver, urea diminishes or disappears from the urine, and in its place appears a proportionate amount of leucin. This is exactly what might be expected if the conversion of leucin into urea habitually depended on the normal activity of the hepatic cells.

It does not, however, follow, whatever may be the proportion of albumen which is converted into leucin, that all the leucin which passes from the intestine to the liver should there at once become converted into urea. It is very possible that the hepatic cells at first merely so modify the leucin as to render it capable of further changes while circulating in the tissues. The existence of urea in the liver suggests that perhaps the last step of the process, like the first, may be accomplished by the hepatic cells.

Schützenberger has shown that albumen is broken up with hydration into various products under the influence of hydrate of baryta. His pupil, Quinquaud, has obtained these same products from many albuminous tissues, similarly treated by hydrate of baryta, under high pressure and temperature maintained for several days. Of all tissues so treated, the parenchyma of the liver gave the most abundant yield of leucin, tyrosin, and urea. "We conclude that the cellular act of denutrition sustained in the parenchyma, is analogous to the splitting produced in the laboratory under the influence of hydrate of baryta, heat, and pressure. * * * The hydration is effected by means of a proteic ferment, which replaces the hydrate of baryta." Quinquaud, Chimie Pathologique, pp. 304–311.

Foster's suggestion is useful in pointing out a supplementary intermediate origin for urea, when unusually large quantities of proteid material have been subjected to the

† See Murchison. Lettsomian Lectures on Functional Diseases of the Liver; also Quinquaud, Chimie Pathologique, Paris, 1880, p. 314. An essay on the formation of urea in the liver, is to be read before the British Medical Association on its meeting of this year.

action of the pancreatic juice. But it does not disprove the conversion of peptones into urea. Nor does it nullify the rôle of "circulating albumen," when this term is understood to include all the intermediate products existing between the protein ingested and the urea eliminated. Between urea, on the one side, and the peptones and leucin * formed in the alimentary canal, on the other, exists a wide gap, occupied by a series of chemical compounds, which must be formed within the tissues, either of some one organ, or, more or less, of all. All these changes, up to the formation of urea, imply the splitting of albuminous molecules into simpler compounds; a process constantly attended by liberation of force.

We have entered upon this rather long discussion, whose problems, after all, we could do no more than indicate, because the complete explanation of the increase of urea observed during the cold pack, is inseparable from the theory of the origin of urea. For us, the very first question to decide is, whether the excess of urea appearing in the urine during the hours of the pack, depended on an increased formation of urea during this same period, or merely on an increased elimination from the recesses of tissues in which it was accumulating.

No less an authority than Ludwig has asserted the increased "washing out" from the tissues, of previously formed urea, as an explanation of the large amount which appeared in the urine on the first days of Voit's hunger experiments. Voit, however, has shown that in such cases no urea could be found in either the blood or the organs; and further, that when urea was ingested, it could all be recovered from the urine within 24 hours.

On the supposition that, during the pack, urea previously retained in excess was washed out from the tissues, we

* And tyrosin.

should expect to find, during the hours preceding the pack, some indication of such retention. But there is no such indication, and no experimental proof, indeed, that when the highly soluble urea has once been formed in the tissues, it fails to be immediately eliminated from the body, so long as the kidneys are healthy.

Variations in the elimination of urea depend, therefore, on variations in its formation. But these may affect any of the steps in the long process by which albuminous substances reach their final stage of oxidation, beginning with the absorption of peptones (and of leucin) from the alimentary canal. Sluggish absorption of albuminoids must diminish the amount of urea in the urine, as decidedly as would the diminished ingestion of proteid material. We have pointed out* several conditions, frequently met with in anæmic people, which render primary absorption sluggish; and have showed how the pack, by increasing the force of the abdominal circulation, may quicken this primary absorption.

But, further, if there be a similar sluggish intervascular circulation in the glands, especially the liver, through which the albuminoids must pass during their progressive metamorphoses, these metamorphoses may be retarded. Though we must doubt the retention of urea in the tissues, there is no reason to doubt the frequent retention of intermediate, less soluble products, when, from any reason, they are not passed rapidly enough from cell to cell, or through the capillaries of the organs in which they are being elaborated. Now, agents which increase osmosis and intervascular streaming, might be supposed to favor these chemical changes, just in proportion as such changes are dependent upon the elementary circulation of the imperfect products.†

* Page 21.

† And there is every reason to suppose that the dependence is very close. Many, if not most clinical phenomena, necessitate prolonged movements upon

Conspicuous among this class of agents are water and chloride of sodium, both of which increase osmosis, and also must accelerate the decomposition of circulating albuminoids, for they both considerably increase the production of urea.

The increased circulation in the abdomen determined by the cold pack, and which we have already shown to be the cause of the diuresis, should produce much the same effect on the formation of urea, as does the ingestion of large quantities of water. In the liver, there will be increased vascular pressure and transudation; and, in consequence of the *vis a tergo* thus applied, an increased circulation from cell to cell. Thus, on the one hand, a greater quantity of albuminoid material brought to the liver, by quickening of primary absorption; on the other hand, such increased interstitial circulation in the liver, as should facilitate the metamorphosis of the newly arrived albuminoids.* As one final result of both, increased production of urea.

If, as has so often been supposed, the urea came from the disintegrating plasma of tissues, its increase, in states of denutrition, must always be considered a misfortune. But coming, as we see no reason to doubt that it does, from the albumen of the food, the urea eliminated may serve in several ways to indicate nutritive assimilation.

In the first place, as we have seen, it may show that, under the same circumstances of diet, more food has been absorbed from the ailmentary canal; part of which *may* have been retained and stored up without decomposition.

In the second place, whenever an albuminoid molecule is split up in the formation of urea, and its nitrogenous por-

one another of the substances which are to be changed by mutual interaction. We see no reason to assume,—as seems to be often tacitly done,—that all the substances which transude through capillary walls remain at once permanently outside them. Rather must we admit a constant in-and-out streaming throughout all the elements of the tissues,—capillaries, cells, conjunctive tissue spaces,—until the product of any tissue has been sufficiently elaborated to be finally removed from it.

* Peptone and leucin.

tion eliminated, there is a possibility that the organism will retain and assimilate the non-nitrogenous portion. Whether, in any given case, this has been done or not, can only be known with certainty after analysis of the products of respiration. But the assimilation may be inferred whenever an increased elimination of urea is found to coincide with improved nutrition and increase of weight.

In the third place, the formation of urea, indicating as it does the decomposition of albuminoids, is in itself a proof, and to some extent a measure, of an evolution of force; and an increased production implies an increased evolution of force. To provide, immediately or remotely, for this evolution of force; to thus prevent destruction of organized albumen for the same purpose, is the principal function of the proteids of nutrition. When, with ingestion of the same amount of albumen, more urea appears in the urine, it is proof that a larger proportion of albumen has been completely burned, and that proportionately, more force has been placed at the disposal of the mechanisms of the economy. To revert to Voit's simile, the stream of albuminous molecules, in which revolve the wheels of the organism, has been deepened, so that the myriad revolutions could be more easily accomplished.

Now, in the anæmias we are considering, the organism is principally suffering from deficiency of force,—from functional debility. The increased development of force effected in its tissues, as testified to by the increased elimination of urea, should ultimately constitute a tonic as positive as alcohol or quinine, but one whose more complex influence is not so immediately perceptible.* But we have seen that whenever the increased elimination of urea continued during the hours following the pack, so that the total amount for 24 hours was increased, the patients were fatigued rather than

* For *immediately* after the pack patients are much fatigued.

invigorated. If the excess of urea continued for several days, decided symptoms of malaise appeared. When the pack was entirely beneficial, the increased production of urea during the pack was compensated by diminished production afterward, so that the daily amount was rather below than above the average for that patient. Thus, the rhythm of the urea excretion offered a fairly correct indication of the benefit that might be expected from the packs.

When the permanent excess of urea was attended with symptoms of exhaustion, the patients, in spite of improved appetite, had not yet succeeded in increasing the daily amount of food, in proportion to the increase of osmosis and of facilities of waste. It is possible, therefore, that the organized albumen of the body was still insufficiently protected by the ingesta, and, under the influence of an increased elementary circulation, was decomposed in excess of what could be easily tolerated.

The diminished production of urea after the pack, may be explained in several ways.

1st. To the extent to which production had been increased by the temporary increase in the abdominal circulation, must it diminish when the habitual equilibrium of the circulation was restored.

2d. Similarly, to the extent to which the excess of urea was derived from increased chemical processes in the muscles, sustained during the production of heat, must it diminish when, by equilibrium of temperature, these processes were temporarily arrested.

3d. The increased elementary circulation, followed at first by increased decomposition of the circulating albumen, may subsequently, though not coincidently, determine an increased movement of assimilation. This movement may include, on the one hand, albuminous materials absorbed in more abundance from the alimentary canal,

and now retained without decomposition; on the other hand, the non-nitrogenous portion of the albumen, which has been decomposed, and whose nitrogenous part has been eliminated as the excess of urea. That the movement of elimination and of assimilation did so alternate, was indicated, not only by the rhythm of the urea excretion, but by the fact that our patients were nearly always tired immediately after the pack, sometimes extremely so, but felt invigorated a few hours later, that is, during the period of diminished production of urea.

We know, as an important result of the respiration researches, that the oxygen consumed by the organism is not absorbed at the moment that it is required for use, but previously; and conversely, that the oxidations constantly going on, are not effected at the expense of the oxygen which is simultaneously being absorbed, but at the expense of that which has previously been stored up.* Consequently, an increased production of urea consumes a larger amount of stored-up oxygen. This increased consumption determined by the pack, like that caused by exercise, is followed (not accompanied) by an increased absorption of oxygen. During this period, oxidations, and consequently the production of urea, must be diminished.

Further: Pettenkofer and Voit have shown that the absorption of oxygen increases with the amount of *organized* albumen existing in the economy. To the extent, therefore, to which the increased amount of circulating albumen (obtained by quickened primary absorption) could be converted into organized albumen, should the absorption of oxygen be increased.†

* See note, p. 166. To use a rather homely simile, "raw" oxygen is of no use to the tissues; it must be mellowed, *i.e.*, rendered active by long keeping. In other words, as has been suggested, it must first enter into combinations, from which it must be subsequently liberated in a nascent state, *i. e.*, as ozone.

† See table of gradual increase of hæmoglobine under the influence of richly albuminous diet, in essay, by Leichtenstern, Untersuch ueber den Hæmoglobin-Gehalt des Blutes, p. 43, Leipzig, 1878.

But if this be so, we discern new reasons for a peculiarly beneficial effect of the cold pack in anæmia. The best-defined characteristic of anæmia hitherto recognized, is the deficiency of hæmoglobine in the blood, whether this be caused by a numerical deficiency of red corpuscles, or by a deficient constitution of corpuscles numerically sufficient.* The significance of this poverty in hæmoglobine depends on the diminished absorption of oxygen which it implies. It is customary to add, "and which it necessitates," looking at the deficient absorption of oxygen as being exclusively due to the lack of hæmoglobine in the blood to take it up. In the light of Pettenkofer's researches, however, we may inquire whether a deficient absorption of oxygen be not itself a cause of the gradual disappearance of the hæmoglobine.

It has been seen that all organized albuminoids *fix* oxygen in a way presenting many analogies with the fixation of oxygen by hæmoglobine, itself an organized albuminoid. Also that the hæmoglobine does not take up the same amount of oxygen whenever exposed in the lungs to the same amount of air, but takes up more at night than in the daytime, and takes up more for individuals whose tissues are rich in organized albumen than for others. If the poverty of these tissues reaches a certain point, or if, perhaps, their condensing activity be diminished in some, as yet, unknown way, we may suspect that the hæmoglobine would then take up less oxygen for carriage,† and, finally, in accordance with a general law, atrophy with the failure of its functional activity.

Unquestionably, in anæmia, the hæmoglobine is atrophied: there is much less than the normal proportion in the blood. Possibly even that which is present is not nor-

* As shown by the recent researches of Hayem and of Gowers.

† 1.76 ccm. for every gramme is the normal amount. Foster, Text-book, p. 274.

mally saturated with oxygen. This deficiency is at the basis of the general functional debility, which, far more than waste of tissue, is the conspicuous morbid symptom of anæmia.

If, therefore, during the hours following the pack, there is an increased absorption of oxygen, determined by the increased consumption of stored oxygen during the pack, the fundamental condition of anæmia tends to be modified.

This oxygen could only pass into the blood combined with the hæmoglobine. If the demand of the tissues for oxygen exceeds the amount which can be carried by the existing amount of hæmoglobine, a tendency is initiated toward the formation of more hæmoglobine, provided the remaining elements of its composition, iron and albumen, are simultaneously furnished in sufficient quantity. Obscure as must at present be our conception of the operation of this, as of any other physiological "tendency," it is at least as clear as that which we can frame of the tendency to the reconstruction of entire blood corpuscles, which is to be initiated by the simple process of introducing more iron into the blood. Yet this tendency has long been regarded as extremely comprehensible, and Hayem's recent essays reaffirm this idea without any better explanation of it:

"Iron is an agent which constantly solicits the blood corpuscles to charge themselves with hæmoglobine."

"The ferruginous medication is therefore one of the most rational in therapeutics." *

Both these remarks are undoubtedly true. Yet not a very wide clinical experience is required to show that this same medication, employed alone, frequently fails. This, not because it is not rational, but because it is not sufficient.

The administration of iron in anæmia encounters the following difficulties:

* Des caractères du sang dans les anèmies, 1876.

1. The frequent occurrence in anæmic persons of gastro-intestinal hyperæmia, which interferes with the absorption of iron, and is itself easily aggravated by its presence.

2. Ordinary food contains enough iron for the maintenance of the blood in health, but in anæmia this ceases to be appropriated. Whatever hindrance exists to such appropriation, must be overcome before the excess, given therapeutically, can be taken up.

3. The construction of the blood corpuscles demands oxygen and albumen as imperatively as iron. To judge from the researches, now classical, of Quevenne, Miahle and others, iron is mainly absorbed in combination with peptone, and in proportion as it produces its primary effect of increasing the secretion of gastric juice and also the amount of peptone dissolved in it. But this effect is not unfrequently prevented, and cannot be produced unless other therapeutic agencies are made to coöperate with the iron.

The preceding pages have sufficiently indicated the various ways in which the cold pack may be demonstrated or expected to aid in overcoming these difficulties. It would be superfluous and outside of our subject to attempt to show the advantage to be derived from the iron, when the various hindrances to its effectiveness shall have been removed. One remark, however, may be permitted.

Although in normal blood the iron exists exclusively in the hæmoglobine of the corpuscles, it seems to be possible, under some circumstances, to introduce more iron than could possibly be so combined. Thus, with the iron treatment of diphtheria, when five minims of tincture of iron are given every 15 or 30 minutes, although it be admitted that some of this escapes absorption, yet the unquestionable efficacy of this particular treatment would not, as it seems to us, be explained sufficiently by local action on the pharynx, and implies that a considerable portion of the iron ingested passes into the blood and, necessarily, into its serum.

It was this medication that suggested to us the possibility of introducing a great deal of iron into the blood by giving soluble preparations of it in minute doses frequently repeated, *e. g.*, a grain of the tartrate of iron and potassa every hour, accompanied by at least a few mouthfuls of food. We ask ourselves whether the iron, which possesses the property, common to the inorganic substances of the economy, of increasing the diffusibility of albuminoids, may not be useful on this account, and apart from its relations with hæmoglobine. In that case it is conceivable that an amount of iron could be utilized considerably in excess of what could be taken up by the blood corpuscles, or even made available in their regeneration.

Among the many complex processes affecting albuminoid substances, which are so largely carried on in the liver, the formation of blood corpuscles, at least in part, must be probably reckoned. Malassez, as the result of comparative measurements of the hæmoglobine of the blood before and after its passage through the spleen, has revived the old opinion of Hewson that the spleen also concurs in the production of the red corpuscles.* In either case, the increased circulation through these great glands, which we have seen to have such an effect on the production of urea, should facilitate the hæmapoietic action also.†

To sum up: Anæmia is a morbid state, characterized by an inability on the part of the tissues to condense oxygen and to store albumen in sufficient quantity. The inability is frequently congenital, or acquired in early childhood.‡ As a first consequence, the reserve material required in the elaboration of force is everywhere deficient. As a second

* Soc. de Biol., 1876.

† *Stasis* in liver or spleen is attended by destruction of corpuscles.

‡ Leichtenstern (loc. cit., p. 1), has shown that the physiological minimum of hæmoglobine in the blood is found in a period of life between the age of six months and five years. Many influences are imminent, capable of prolonging this physiological anæmia into adolescence.

consequence, this elaboration of force is deficient,—there is a generalized functional debility.

The atrophy of the blood corpuscles, or of their functionally active portion, hæmoglobine, is not an isolated lesion, and alone characteristic of anæmia. It must rather be considered as the most easily demonstrable illustration of a disorder common to all the organized albuminoids of the body.*

The cold pack meets the following indications for the treatment of anæmia thus understood:

1. In the first moments of application it produces the same stimulation of the peripheric nerves as may be caused by any application of cold,—shower-bath, douche, plunge-bath, etc.

2. It impresses upon the mass of circulating blood a profound movement of oscillation, first from without inward, then the reverse. The effect is different in the two periods.

During the inward movement of the blood, the tension of the abdominal blood-vessels, which has at first been lowered through the agency of the depressor nerve, at first relaxed, becomes raised by the increased volume of blood driven to them, and circulating through the abdominal viscera, not with increased rapidity, but with increased force. As a consequence there is:

a. Increased metamorphosis of albuminoid substances in liver and spleen, resulting finally in greater production of urea. When iron is absorbed with the albumen, there seems to be initiated in these same glands more abundant regeneration of red corpuscles.

b. Increased consumption of stored or latent oxygen in the series of oxidations culminating in urea. Hence, during the period following the pack, probably increased ab-

* These two statements are deductions obtained by comparing the clinical phenomena of anæmia with the results of various physiological researches. It would be very desirable to test their validity as far as it might be done by means of respiration researches with Pettenkofer's apparatus.

sorption of oxygen, coinciding with diminished oxidations. The latter are indicated by diminished production of urea. (Of carbonic acid also ?)

c. Possibly increased movement of assimilation of now decomposed albumen (and other food), coinciding with the movement of increased decomposition, affecting that portion of circulating albumen which has originated the urea. Both movements immediately dependent on an increased force of elementary, intervascular circulation.

d. Probable assimilation of the non-nitrogenous portion of the decomposed albumen.*

e. Increased elimination of water from the kidneys, and hence, aspiration of excess of water from anæmic tissues.

f. During this elementary outstreaming of water, facilitated washing away of acid fatigue-products from nerves and muscles.

This latter (calculated) effect, to be attributed partly to the second half of the movement of oscillation of the blood mass. During this secondary movement from within outward, we have:

A. Diminution of passive hyperæmia in the alimentary mucous membrane.

B. Increased nutritive absorption, partly in consequence of allayed hyperæmia, partly as the direct expression of a movement of fluids outward from the alimentary canal.

C. Afflux of blood to muscles, enabling them to increase their store of contractile material, and thus become more capable of exercise.

D. In this afflux, and on account of thermic irritation of the peripheric nerves, increased production of heat. From the coincident immobility of the body, and the arrest of radiation, a certain proportion of this increment saved. (The increment of urea is probably derived, in part, from

* This is to be inferred, if the elimination of CO_2 should be found unchanged or diminished during the period of increased production of urea.

increased chemical changes of circulating albumen in the muscles, during the production of heat.)

E. In the production of heat in response to a physiological stimulus, the nervous system, through the portion involved in the reflex mechanism, is especially stimulated, and the stimulus is immediately followed by special provisions for repose.

F. During the afflux of blood to the periphery, blood is drawn from the nerve centres, which are thus placed in a condition analogous to sleep,—a condition favorable to repose and to nutritive assimilation. The establishment of an equilibrium of temperature is followed by a cessation of chemical activity in the muscles, and necessarily by sedation of the nerves. These effects are of especial symptomatic importance in irritable anæmias.

3. During the pack the radial pulse is slackened, and its tension lowered. We may infer increased facilities for nutrition in tissue-elements hitherto irritated rather than nourished by a blood-stream imperfect in quantity and too rapid in duration.

Massage intensifies and prolongs some of the effects of the pack, when this has previously been administered.

Given alone, it is much less effectual than the pack, because its influence is less complete, and especially because it is less certain to determine blood to anæmic muscles.

In cases of "neurasthenia," or of hysteria, the cold pack is only beneficial in proportion to the coexisting anæmia. If this is not marked in proportion to the neurotic element, the pack may be useless or even injurious.*

The cold pack is decidedly dangerous, if administered too near to periods of abdominal hyperæmia, whether physiological, as digestion and menstruation, or pathological, as in lurking peritonitis.

* In several cases of hysteria, in which the hæmoglobine was measured by Quinquaud (*Recherches d'Hématologie Clinique*, Paris, 1880), the quantity was found to be normal.

THE USE OF QUININE WITH NERVOUS SEDATIVES.*

By LANDON CARTER GRAY, M.D.,

LECTURER ON THE NERVOUS SYSTEM, LONG ISLAND COLLEGE HOSPITAL.

WIDESPREAD as is the use of the bromides, the proper indications for their use are not, in my opinion, always as well understood. The general idea in regard to them would seem to be, that they are quieters of overaction of the nervous system; but it is seldom that a man is found who makes a distinction between the kind of overaction that they will favorably influence, and the kind to which they are either negatively or positively deleterious. A little reflection will make it evident that this so-called overaction may be produced by various pathological conditions that are diametrically different in their causation, and which will therefore call for as varied methods of treatment. A neuralgia, for instance, may be set agoing by a hyperæmic condition of the nerve or its centres, or by an anæmic or ischæmic condition of the same; or it may be started by the impure blood of a gouty or rheumatic individual, or by malaria, syphilis, impure air, heat, cold, improper food, overstraining of the eyes; or, being purely functional, it may occur in a patient who is robust and ruddy, or in one who is debilitated. A tremor may be seen in persons whose molecular conditions are the

* Read before the American Neurological Association, June, 1880.

feeble ones that obtain toward old age, as in paralysis agitans; or the sufferer may be the younger and stouter subject who usually bears about a disseminated sclerosis. An epilepsy or an insanity may occur in a feeble man or woman, or in those whose physical condition is otherwise above reproach. *A priori* it would seem improbable that any one remedy, as the bromides, would be equally efficacious in all these conditions. And so it is. A distinction should be made in the application of these salts. If we adopt a classification that has, in a large measure, lost its favor in these days of stimulation in medicines and foods, —that, namely, into sthenic and asthenic cases,—we shall, if I may be allowed to speak from my experience, be possessed of a reliable and rational criterion. We may expect benefit from the administration of the bromides in individuals whose hearts beat well, who are ruddy, who are not in need of tonics or stimulants, who are *sthenic*. We must not so confidently anticipate relief in those who are debilitated, who are *asthenic*. My attention was first called to this distinction by some unfortunate experiences. A young but confirmed epileptic was placed under my care. He was greatly demented, thin, haggard, feeble. I ordered for him ten grains of the bromide of potash and five grains of the bromide of sodium morning and noon, and double the dose at night. He had taken the medicine for a week, with such apparently happy effect that the parents were gushing with gratitude at the improvement of mind and the great diminution in the convulsions. I directed that the doses should be doubled. The next day I was summoned in great haste, and found my patient, who had taken one increased dose, in a state bordering on collapse,—pupils enormously dilated, radial pulse almost imperceptible at the wrist, extremely pale, dribbling saliva, respiration slow and sighing, semi-comatose. I succeeded in bringing him out of danger; but I never saw him again.

I had an opportunity of observing a case of paralysis agitans in an old man over 70, whose physical condition was fair, and for whom potassium bromide, ten grains, and sodium bromide, five grains, were prescribed thrice daily. For several weeks he improved considerably. He then became suddenly very feeble, shortly afterward was paralyzed upon one side, and died within a week or two. I have no means of knowing positively that this untoward result was due to the medicine, but from similar cases, some of which I have heard, others of which I have seen, I strongly suspect that such was the fact. In cases of neuralgia, too, about the head and face, in cephalalgia, in insomnia, in cerebral exhaustion, in many forms of nervous irritability, I have again and again witnessed the administration of the bromides producing either no amelioration at all, or giving only temporary relief that was soon followed by increase of the trouble, or doing actual harm.

But there are certainly patients with whom it may be desirable, even though they be asthenic, to obtain the peculiar sedation of the bromides in all its fulness. In treating epilepsy, for instance, I am a firm believer in the efficacy of the bromides. But, day after day, as patients have walked into my clinic, or my office, listless, with dilated pupils, feeble pulses, lack of appetite, pale, drowsy, I have been struck with the thought that the improvement in the epileptic symptoms was being gained by a method of treatment which was only justifiable in the absence of any less depressing therapeutical procedure. I began to use quinine for its tonic effects; in small doses, however, and cautiously, for I feared that it would lessen the restraint of the salts upon the disease. To my *surprise, however, I found that not only did it not do so, but that whilst it decreased or dispelled the bromism, it increased the anti-epileptic potency of the bromides.* I then prescribed the quinine in larger doses, and obtained similar results in a more marked manner. I have

now pursued my investigation of the subject for upward of two years in a large number of epileptics, and I am convinced of the actuality of what I first observed. When exhibited with the bromides in other diseases, as in insomnia, certain forms of neuralgia, headache, and the manifold nervous irritations which accompany so many pathological conditions, quinine will in like manner remove the bromism and increase the effect of the salt over the disease for which it is given, unless the patient be one of those upon whom quinine acts injuriously, because of plethora, lithæmia, idiosyncracy, or some other cause. It has now become my habit in treating epilepsy to give the bromides until bromism is produced, then to give quinine in doses of two to three grains twice or thrice daily. In some cases this may answer the purpose, but in the greater number who will require still larger doses of the bromides, the latter are better borne with the quinine. There are some exceptional epileptics of excellent general health to whom quinine seems injurious, and who bear well the full sedation of the bromides, but the larger proportion of epileptics do not belong to this class. In treating other nervous troubles, occurring in individuals of the asthenic type, in whom I desire to obtain the calm that comes from the bromides without their depression, I first get the stimulation of the quinine before the bromides are prescribed.

Quinine, when given in conjunction with belladonna in epilepsy, will also deepen the effect of these drugs upon the nervous system, at the same time that it decreases or dispels their depressing action. I have tested it with belladonna in many epileptics. I have also tried it with hyoscyamine in several cases of paralysis agitans, in several cases of insanity in my practice, and in a larger number in those wards of the Flatbush Lunatic Asylum which my friend and colleague, Dr. John C. Shaw, has courteously placed under my charge. In a case of paralysis agitans, which I

have elsewhere related at length,* I found, that by the combination of the remedies, I was enabled to increase the action of the hyoscyamine over the disease, whilst I removed the unpleasant headache, slight delirium, flushing of the face, and general *malaise*, which had attended the use of the remedy before the quinia was employed. In this manner I have succeeded in greatly lessening the tremor, contracture, difficulty of speech, sense of heat, feeling of restlessness, etc., and for some two months the patient has been taking his medicines upon this plan, not needing to see me for weeks at a time, and has derived the only relief that has been obtained during a duration of the disease for fifteen years.

In several lunatics, from whom the effects of a dose of hyoscyamine was passing off, or who were becoming tolerant of the drug, so that they were returning to the excited condition for which the drug had been given, I have seen them calmed again by five grains of quinine, although none of the listlessness and languor of the hyoscyamine would reappear. The result of the combination was particularly happy in a case of puerperal mania† which I saw in consultation with Dr. James R. King. The patient, who was greatly excited, was fully brought under the influence of hyoscyamine, with only temporary effect. I was then a novice in the use of this medicine, and hesitated to prescribe large doses, although the woman was not especially debilitated. I suggested giving ten grains of quinine with the next morning's dose of hyoscyamine, and repeating it the following evening. Dr. King soon wrote me that entire recovery had followed the second dose. It is proper to say that there was no evidence of malaria in this patient, and that large doses of quinine had preceded the hyoscyamine. Let me say, in passing, that hyoscyamine should be prescribed with caution in

* ARCHIVES OF MEDICINE. "Notes on Hyoscyamine," June, 1880.

† I use this term in the technically alienistic sense of the word, whose equivalents are the German *Tobsucht* and the French *manie*.

asthenic cases, not because it is dangerous to life except in extreme weakness, but because it is deleterious in other ways. Belladonna in full doses produces much less depression than either hyoscyamine or the bromides; still I have found it useful to stimulate its action with quinine in asthenic patients.

For these reasons I believe that I am correct in stating that quinine will increase the sedative effect of the bromides, belladonna and hyoscyamine, whilst it simultaneously decreases or dispels the depression which these medicines usually produce. I am fully aware of the difficulty of accurately expressing my meaning in current therapeutic terms, and am thus forced to make a somewhat arbitrary distinction between sedation and depression, meaning by the former term, the calming power over the diseases for which the trio are generally given, and by the latter, the weakness of the heart, respiration, pallor, anorexia, languor, which they generally cause in variable degree. Is this fact one of the many that constitute a law? Do tonics and stimulants generally increase the sedation and lessen the depression of nervous sedatives? I am endeavoring to work out an answer to these questions, to which I am inclined to reply in the affirmative. Among other facts, I am inclined to regard Dr. E. C. Seguin's treatment of epilepsy by the bromide and chloral mixture as another illustration of some such law. It will be remembered that by adding the chloral, epileptics improved, and the bromism was lessened. The objection will be made, of course, that chloral is a nervous sedative itself; but I have usually found that chloral is most likely to succeed in those persons who stand in need of tonics or stimulants.

What the explanation of these facts is, it is not easy to say. It must largely be a matter of speculation; and into that wide realm I prefer not to enter at present.

NEW BOOKS AND INSTRUMENTS.

Contributions to Orthopædic Surgery. By Jos. C. Hutchison, M.D., Visiting Surgeon to the Brooklyn City Hospital, Surgeon-in-chief to the Brooklyn Orthopædic Infirmary, etc., etc. 8vo, pp. 121. G. P. Putnam's Sons, 1880.

The author of this *brochure* presents for our consideration two subjects. The first and more important is what he has termed the "physiological method" of treatment of chronic diseases of the joints of the lower extremity, and the second comprises a series of lectures upon club-foot.

Dr. Hutchison makes two important statements which we may regard as the basis of the "method" he advocates for the relief of chronic articular diseases. The first is as follows: "It is, to my mind, a pathological refinement, in most cases of joint disease, especially in childhood, to attempt to describe the symptoms indicating distinct pathological states of the individual structures composing a joint. The treatment would be essentially the same whether one or all of the articular structures are contemporaneously involved." The second is: "* * * that *there is no extension made by the instrument* (Taylor's or Sayre's on the treatment of hip disease) as the inventors claim, because the strap which is designed to produce extension and passes from the ends of the adhesive plaster beneath the extension bar, is slackened at every step." There are many other minor statements which are interspersed throughout the work, but which we cannot discuss except incidentally on this occasion.

The criticism that traction is lost when the patient bears his weight upon the perineal pads in stepping, when the Taylor-Sayre splint is used in the treatment of morbus coxarius, is a legitimate one, and we have called attention to it on many occa-

sions, long before Dr. Hutchison placed his comments in type. But our own criticism was always tempered by the statement, which our author does not seem to have considered, that during the time that extension was thus lost, the entire limb was controlled in every other respect, and that any tendency to flexion or other malposition of the thigh was overcome. In other words, when traction is momentarily lost, the joint is simply fixed. But when traction exists, the patient has the advantage of that peculiar and perfect immobility which the extension of the long hip-splint affords. By the "method" advised by Dr. Hutchison, nothing is suggested which prevents the various progressive deformities which are very sure to occur unless they are controlled by some mechanical device. A very important objection to the use of our author's plan—even were it perfect in other respects is the ease and frequency with which patients lay aside their crutches for the purpose of playing or sitting down. It has often happened that patients would throw their crutches aside when unobserved. And it must be apparent to all that when a patient occupies any but the *erect position* the "physiological method" ceases to be of service. On the other hand, if portable mechanical instruments are properly used, it makes no difference what position the patient assumes. The relation of the diseased joint and limb to the body is always the same, and the position can be modified at will by the surgeon, but not by the patient. With Buck's extension, which has many advantages over the "physiological method," the patient assumes the supine position, and the entire weight of the body forms the basis of counter-extension. But with a proper extension apparatus, the counter-extension is applied directly to the tubera ischii, and the patient cannot vary the force applied. The use of the splint insures protection to the joint at all times, and when a cure results, there is little or no deformity, even if anchylosis occurs. During the process of repair the patient has been rendered comfortable,—much more so than he could have been by the use of crutches which, in our experience, more so than any splint we have ever seen (and we have seen a good many awkward ones), render the patient an object of commiseration.

Our author assures us, and we can reinforce his assertion, that anchylosis need not be feared as a result of prolonged fixation of the joint. It would be much better for our patients could we, in the majority of cases, secure anchylosis at will by mechanical treatment. But the methods that would produce this result in

healthy joints are not operative as applied to *diseased* joints. Unless reparative effort takes the place of the regressive changes which characterize most of these joint lesions, anchylosis cannot be induced. One riddle which orthopædic surgery is trying to solve, is how to bring about this reparative effort. It would require, with our experience, a far more convincing demonstration than that which our author gives, to lead us to believe that his "method" secures any control over the pathological condition met with, especially in chronic articular osteitis.

We have recently devised, and have introduced to the profession at our clinics at the Orthopædic Hospital, an improved hip-splint which meets the principal objection that Dr. Hutchison makes to the Taylor-Sayre instrument. A description and cut of this splint may be found in Reynder's catalogue of recent date. We have used it with success, and regret that circumstances have interfered with its introduction to the profession through the medical journals. By this instrument constant and continuous traction can be maintained, and the affected joint is not alternately exposed and protected at every step.

There can be no doubt at all, it seems to us, that the more nearly we can secure immobility in the treatment of chronic articular osteitis the more clearly do we meet the indications. Our author very properly insists upon this, though he by no means stands alone regarding his views upon the subject. It is true that some of our pioneers in orthopædic surgery, after having established "a surgical principle," in their own imaginations, have endeavored to impress others by their enthusiasm rather than by their logic. They have had the satisfaction of seeing their views quoted and endorsed by many others. But these errors cannot endure, nor should they affect our estimation of the real principle. Nor should the fact, that too much has been expected of traction in the treatment of chronic joint disease by these enthusiasts, be permitted to weigh against its undoubtedly great value in the management of these typically chronic conditions. Certainly our author goes too far, and he draws altogether the wrong conclusion when he asserts, in italics, that "*immobility and extension are obtained in spite of the apparatus used.*" We made an experiment several years ago, which demonstrated beyond a doubt that the Taylor-Sayre instrument produces almost complete fixation in a healthy hip joint. The experiment, in brief, was the application of a hip splint to the *right* lower extremity of a child, whose *left* hip was anchylosed from former disease. After the

application of the splint to the healthy limb, the patient could not walk one step. All motion at the *left* hip being lost, the boy, with extension, was unable to bring the flexible vertebral column to his aid. When the knee-cap was loosened and flexion of the thigh was permitted, the patient walked very awkwardly for a step or two.

So far as the treatment of chronic articular osteitis is concerned, we may wholly dismiss "extension with motion" from orthopædic surgery. In chronic synovitis "extension with motion" is just as plainly indicated as it is contra-indicated in the former condition. While, therefore, agreeing with Dr. Hutchison in his remark that with the use of apparatus "the whole pelvis swings, and there is no motion at the hip joint," we regret to see him attempting to uphold his "method" by a criticism of the fallacy so long taught by Taylor and Sayre, that the Davis principle of extension afforded motion at the affected hip joint.

Our author further states that the immobility of the joint is secured not by the apparatus employed, but "by reflex contraction of the peri-articular muscles, aided by intracapsular effusion, and by the voluntary effort of the patient to keep the joint at rest on account of the pain which motion produces." He further says: "Fixation of the joint is one of the earliest symptoms and is a characteristic condition of morbus coxarius. It is so marked that when we move the limb the pelvis moves with it,—there is *apparent* anchylosis."

We cannot understand why Dr. Hutchison should permit such statements to go upon record. In how many cases is immobility of diseased joint produced by "reflex contraction of the peri-articular muscles," and the other means mentioned? In how many instances is "fixation of the joint one of the earliest symptoms" of morbus coxarius?

But although the joint, as asserted by our author, is thus rendered immobile, he distinctly states that a certain degree of extension is necessary, thereby agreeing in principle with those who use apparatus. The author's method consists in placing a high sole under the sound side and giving the patient a pair of crutches. "Extension is made by the weight of the limb." Has not this been the treatment for ages? Does it differ materially from that pursued before surgeons rescued these cases from empiricism? Does it accomplish anything to meet the actual pathological conditions, many and the most important of which our author ignores? Is there anything about it, in short, which entitles it to the name of "method"?

In at least 90 per cent. of cases of chronic joint disease, taken as they come, it will be found that a greater or less degree of motion can be detected at the diseased articulation. We never saw a case where "fixation of the joint" was "one of the earliest symptoms." And these facts mean that, in all but exceptional cases, the patient can voluntarily move the affected joint back and forth while using the high shoe and crutches. But the great fallacy of this "method" is that the weight of the limb does *not* act as an extension force. "The limb is never straight," remarks our author. If it were straight, the weight of the limb would act directly, and a certain amount of joint protection would be afforded. But in hip disease the thigh is always flexed. The weight of the entire limb forms a *resistance* to the *power* exerted through the flexors (principally the *psoas magnus*), and the ball-and-socket joint becomes a *fulcrum*. If the weight of a body be 60 pounds, and the weight of the limb is 12½ pounds, as stated by the author, what would be the force expended upon the inflamed hip joint, with the patient erect, using the high sole and crutches?

Our author states that the extension splint is "cumbrous and uncomfortable." It may have proved so in his experience, but it has never been so in ours. We have applied the long extension splint to patients of all ages and both sexes. It has been very exceptional, even when applied to young ladies, to hear complaints. *The relief afforded by a splint, properly adjusted, more than compensates for the slight discomfort it occasions.* We always insist also that the splint should be worn day and night. We have no hesitation whatever, therefore, after an experience which includes nearly six years in the treatment of chronic joint disease by a method very similar to that which Dr. Hutchison advances, in stating that the scientific mechanical treatment of joint diseases accomplishes what the "physiological method" cannot accomplish. We can only reiterate here what we have frequently said at our clinics, that he who neglects to use properly selected and properly constructed apparatus in the treatment of chronic joint disease, fails to afford his patient the best prospect of relief and cure.

Our author cites several cases of hip disease,—seven in all,—treated by the "method" he advises. We regret very much that his modesty prevents him from submitting more of his own, rather than a stranger's cases, which he has never seen. And we must be excused if we raise a question as to diagnosis in certain cases,

and ask if others have really recovered. The inefficient manner in which the cases are recorded renders them useless for scientific purposes.

Dr. Hutchison advises the same treatment for knee- and ankle-joint diseases, as though the necessity for an extension force diminished in direct ratio to the remoteness of the affected articulation from the pelvis. No cases of knee- or ankle-joint disease are reported.

The author facetiously remarks regarding himself, that since he has introduced this "method" "Othello's occupation's gone." We heartily wish that Dr. Hutchison's plan of treatment was all that is claimed for it, and that it was so easy a matter to cure chronic joint disease. Our own experience, however, is exactly opposed to our author's, and of late our own "occupation" has been made more than usually active by the reception of patients who have tried and abandoned the "physiological method." We have no hesitation whatever in asserting that they have all been injured by the experiment, and that marked improvement and great comfort have followed the use of apparatus.

Our remarks have become so lengthy upon the interesting questions involved in the study of the chronic joint conditions, that we can only briefly notice the chapters on club-foot. We have read them attentively, however, but fail to find any novel suggestions. Many recent ideas, and procedures which are in practical operation, are not referred to. The author presents, however, a very readable and concise manual on the treatment of talipes, as practised by himself. As such it is worthy a place in every surgeon's library. [N. M. S.]

Fracture of the Patella; a study of 127 cases. By FRANK H. HAMILTON, A.M., M.D., LL.D., Visiting Surgeon to the Bellevue Hospital, Consulting Surgeon to the Hospital for the Ruptured and Crippled, etc. Charles L. Bermingham & Co., New York, 1880, pp. 106.

It may seem to many a work of supererogation to write a book of 106 pages upon apparently so simple an injury as fracture of the patella. No injury involving an integral part of so important a joint as the knee can be considered trivial. The almost endless ingenuity displayed in the devising of apparatus and of suggestions for treating this fracture, since the times of the Father of Medicine, would seem to compel us to draw one or all of the following conclusions, viz.: (1) The indications for the treatment of this

injury are not properly understood ; (2) the various means at our disposal to fulfil these indications are inefficient ; (3) they are all equally efficient ; (4) we do not correctly appreciate what end is feasible and desirable to be obtained.

No one will attempt to maintain the truth of the first. As to the second and third, it is difficult to decide, when we reflect that the methods of treatment vary from the practically "do nothing" plan of Flajani to the complex apparatus of Burge.

Whether or not we should run even the slightest risk in the endeavor to obtain bony union, whether it is feasible, and whether, if obtainable—an exceedingly dubious thing—it is any better than a short ligamentous union, still remain among the open questions of surgery.

Dr. Hamilton's present work seems to go far toward settling some if not all of these questions.

As the result of the ripe experience of a surgeon who has enjoyed exceptional opportunities, this monograph could not fail to be a useful addition to our surgical literature. It possesses, however, a unique merit in giving us the materials from which the author draws his conclusions, with a sufficient fulness of detail to enable us to correct them should they be erroneous.

When we consider that of 10,995 cases of fractures observed at the Hotel Dieu and the Pennsylvania Hospital, only 193 were fractured patellæ, the 127 cases recorded in the present work form no mean contribution to our knowledge of this injury.

It is a matter of regret that such a number of these are more or less incomplete as to termination, etc., but even such imperfect records are of value as illustrating the site, cause, or primary results of the injury.

The author opens with the *raison d'être* of his book, viz., the absence of a study of a large number of recorded cases of fractured patellæ, and his own exceptional opportunities. He then calls attention to eleven points which have specially struck him : "(1) The large proportion of simple transverse fractures, and the infrequency of comminuted and compound fractures. (2) The frequency of fracture from muscular action. (3) The frequency of early joint effusions. (4) The difficulty which has constantly been experienced in securing and maintaining apposition of the fragments. (5) The great variety of methods which have been adopted, and the frequent changes made in the treatment of the individual cases, either because of their inefficiency, or because of the pain and excoriations or other more serious injuries which

they have occasioned, and the equally good results where the attempts to get close union have been less assiduous. (6) The uniformity of a fibrous union with some separation. (7) The frequency of a re-fracture and its more serious results. (8) The frequency of anchylosis, and its proportion to the time the limb is kept in splints. (9) The great time that elapses before the functions of the limb are restored. (10) The inadequacy of the ordinary knee-caps while the patients walk about. (11) The remarkable power of restoration of the functions of the limb after a time, when no union of the fragments has taken place, if only the patient continues to use the limb, and thus develops the muscles." The remainder of the work is divided into four papers, the first containing the records of cases coming under the author's own observation—55 in number—covering 30 pages, illustrated by six wood-cuts. Next follows, in the second paper, the notes of 72 cases, drawn chiefly from the records of Bellevue Hospital, with a few from those of the Reception Hospitals and from the case-books of other surgeons. These occupy 47 pages. The third paper, covering five pages more, is devoted to an admirable summary of all the cases. The fourth and concluding paper occupies the remaining eighteen pages, "being the substance of two lectures on fracture of the patella, delivered at Bellevue Hospital, November, 1879."

Having now given a general outline of the work, an attempt will next be made to analyse, as thoroughly as space will permit, the last two papers, which contain all that it will prove profitable to examine critically. Turning then to the third paper, we note the interesting fact that no cases are recorded, caused by muscular action, under twenty years of age. Three, aged respectively five years, sixteen years and nineteen years, are indeed related, but they were all the result of direct violence. In the last case a small new patella was formed in the inner part of the ligament, which was much elongated owing to the rupture of its outer portion. The author, therefore, infers that in youth "muscular action seems to take but little or no part in the production of these fractures."

At twenty years, four cases are mentioned. In one, the fracture was the result of direct force, in another, the cause was unknown, and in the other two, the injury was produced by muscular action. The third and fourth decades are the most prolific in this injury, giving in the aggregate 81 cases, its frequency decreasing with each succeeding one, until from 60 to 80 inclusive, we find re-

corded but 5 cases, the majority, if not all of these, being produced solely by direct force. This absence of the muscular factor in the production of fracture of the patella in advanced age, is exactly that which *a priori* we should expect from the enfeeblement of the muscles. But 11 were other than simple fractures, 9 of these being comminuted, and 2 were, in addition, compound, and complicated by other injuries. The direction of the fracture was transverse in 106 cases, the exact point being mentioned in only 45 cases, viz., 22 below the middle, 16 at the middle, and 7 above the middle of the bone. In 83 cases the cause of the fracture has been noted, and in 45 this point has not been alluded to; 25 were the sole result of muscular action, while 58 were ascribed to falls on the patella. "Of 4 oblique fractures, 3 are known to have been from direct force, and all of the comminuted fractures, except Case 127, were from direct blows, as were also the 2 compound fractures." Acute synovitis was noticed in a large proportion of the cases, and was probably present in many others. Every conceivable plan of treatment was used. A noticeable fact, however, was, as the author points out, that the surgeon in most cases changed the method once or more during the course of the treatment, so as to make it doubtful how much credit was due to any one apparatus. Where fixed dressings, such as plaster of Paris, were used, some cases were allowed to go about on crutches. In most, the rule has been the recumbent position, with the foot elevated.

Dr. Hamilton's analysis of the results is of great interest. In 84 cases the union was fibrous, and in 3 or 4 others, owing to the impossibility of moving the fragments the one upon the other, Dr. Hamilton thinks it *may* have been bony. In 3 cases no union occurred. The length of the bond of union is given as "¼ of an inch in 16 cases, ½ in. in 33 cases, ¾ in. in 13 cases, 1 in. in 3 cases, and 5 in. in 1 case. The last 4 cases, or those in which the separation exceeds 1½ inches, are respectively cases 22, 23, 54, and 111. The above records, it will be understood, do not include cases of rupture subsequent to union, but only the results of the first treatment." Anchylosis was always noted to some degree on removing the dressings, especially when the treatment was prolonged and passive motion omitted; but it always eventually disappeared *without forcible movement of the joint.* In a little over one-fifth of the cases, the new ligament has given way; of which number five yielded gradually. This secondary injury occurred generally about the seventh or eighth week, eighteen

cases happening within ten weeks of the primary accident. It has generally been the result of a slip whereby the joint has been suddenly flexed. Later than the eighth week, only eight cases are noted and a doubtful ninth. Of these, four occurred at three months, three at five months, and one at two years and four months, the ninth case having no reliable history. Dr. Hamilton, therefore, concludes that after five months "there is no more danger to the injured limb than to the sound one." Displacements, other than longitudinal separation are mentioned, but merit no special description.

Owing to weakness of the injured limb, an undue strain is put upon the sound one, and in five or six of the cases the other patella has given way.

Our author considers that the hypertrophy of the fragments, noted in nine cases, is probably present more frequently than the notes would indicate.

The most important point of how soon the injured limb regains its usefulness, is answered by Dr. Hamilton's careful examination of 23 cases, where he finds that although, in general, work has been resumed at the end of one year, the full use, if it can be called full, was not obtained until the expiration of two years from the time of injury.

When the leg has given way soon, viz., within three months, four-fifths have terminated very satisfactorily, treatment being immediately resumed; but when the fracture has occurred more than once, and when the secondary injury has occurred later than three months, reunion has failed. Dr. Hamilton, in the opening of the fourth paper, states that the result of the foregoing analysis has for the most part confirmed his previous opinions, but that in some points the conclusions to be drawn are "new and surprising."

As to the etiology of patellar fractures, Dr. Hamilton points to the large number known to be caused solely by muscular action, and then states his belief that this cause was "more or less sufficient," "in all of the simple transverse fractures, and in at least one of comminuted fracture,"—in other words, in all but twenty of the cases. After stating as reasons the preponderance of the accident in males, the age, etc., he states that he has never been able to produce a transverse fracture on the cadaver. The direction of the fracture, as shown by specimens, indicates a "cross-strain" as the rupturing force. "That the bone breaks most often in the lower third, may perhaps

be explained by some mechanical law, but I am not prepared to explain it."

Although from the notes, it is impossible to state the exact position of the joint when the fracture took place, yet it may be fairly assumed to have been semi-flexed, or thereabouts; *i.e., the position in which the greatest muscular force is exerted.*

An examination of the patella will show that it has, besides the two main lateral facets for either condyle, two faintly marked transverse lines marking off three pairs of facets. The uppermost rest on the condyles during extreme flexion; the lowest during extreme extension, *and the broadest middle pair rest fairly on the condyles during semi-flexion*, leaving the projecting apex of the bone without any support; *i.e.*, the lower third and weakest portion of the bone to be broken by the cross-strain. We are somewhat surprised that Dr. Hamilton was not aware of this mechanical fact; one, too, so easily capable of demonstration. The comminution produced by muscular action is explained by the fibres of the two vasti, which are separately attached to the bone, continuing to act after the rectus has broken the bone transversely. Our author clearly indicates the source of error in considering the fracture to be due to direct force, because the patient falls on his knees. In this position the tubercle only,—not the tuberosity of the tibia as Dr. Hamilton calls it,—can touch the ground. The bone is usually broken *before* and *causes* the fall. If a man falls prone, with the knee slightly bent, the patella may strike the ground; otherwise it cannot. Doubtless, the shock of the fall causes a spasmodic contraction of the quadriceps which may break the bone. This is the way in which Dr. Hamilton thinks a fall on the knee may cause a fractured patella.

Generally "the lesion is limited to the bone; its periosteal coverings, including the synovial membrane and the thin, scattered fibres of the tendon of the quadriceps which traverse the front of the bone." We are at a loss to understand what is meant by "its periosteal coverings including the synovial membrane," as the latter certainly does *not* cover the patella at all, except, perhaps, during fœtal life; unless the doctor refers to the synovial bursa situated in front of the patella.

As only a few fibres of the aponeurotic expansion formed by the tendons of the two vasti muscles are usually ruptured, the fragments do not separate more than one inch; indeed, generally are only one half that distance apart. When the separation exceeds one inch, the aponeurotic rupture must be extensive, as is

demonstrated by a dissection which the author relates. Dr. Hamilton thinks that in the majority of instances, the bursa in front of the knee has its posterior wall ruptured, and thus communicates with the knee joint. The usually slight laceration of the aponeurotic expansion of the vasti tendons explains the power of walking after the injury, enjoyed by a few of the patients, as these tendinous fibres are eventually attached to the epiphyseal line of the tibial head.

Dr. Hamilton considers that the rapid swelling of the joint immediately consequent upon the injury, is due to the effusion of blood into its interior, which is subsequently absorbed. The synovial effusion, which almost inevitably results, "cannot usually be detected after the second week;" but the author points out the important fact that in many cases a "more or less extensive cellulitis ensues, involving the front and sides of the knee, and extending some distance up and down the limb." This usually is of no moment, but when injudicious pressure has been used, unpleasant results have followed.

Dr. Hamilton distinguishes, in his section on prognosis, between the ultimate results of the primary accident, and those of the refracture of the uniting bond. He says that in the primary cases, owing to the shortness and hardness of the uniting medium, when the treatment ceases, bony union may be considered present, but eventually the fragments separate, or become movable upon one another, thus demonstrating the error. He does not deny the possibility of bony union, but has never seen such, and believes it most likely to occur in the vertical and comminuted, rather than in the transverse fractures. He thinks that the uniting ligament usually about equals in length the original space between the fragments, and that in most cases it may be rendered *less* by appropriate treatment. When the separation has been great, in some cases the bond has proved not much longer than the average, but in others it has been very long, or non-union has occurred.

Space does not permit examining in detail the slight "tiltings," etc., of the fragments, and the interesting mechanical explanation of these displacements, but it is as well to mention that the author considers that they "contribute their proportion to the subsequent maiming." The slight *lateral* displacement noted in a few cases after use of the limb, has rectified itself.

Dr. Hamilton considers that if the uniting bond be not over one inch its being fibrous is "probably of no consequence;" if greater

than this, however, the functions of the limb are apt to be impaired. Owing to the rarity of *ascertained* bony union, the question of its greater or less liability to re-fracture, compared to a ligamentous one, cannot be ascertained; "my conviction, therefore, is that a fibrous union of less than one inch in length is quite as advantageous as a bony one, but I do not state this as an established fact."

The large proportion of re-fractures, or yielding of the new ligament,—27 in 127 cases,—first indicated by the analysis of these cases, the author emphasises to warn both surgeon and patient to use the limb with great caution until after three months have elapsed. The prognosis of such cases of re-fracture is very unfavorable, the majority refusing to unite. When they resulted favorably, the primary treatment was immediately resumed. Even where either union failed, or the new ligament was very long, time and judicious treatment availed much.

Under the head of treatment, the following are the most important conclusions arrived at: "That under any plan of treatment fibrous union can only be expected, and that when this does not exceed one inch it is as useful as a bony union;" that only Malgaigne's hooks, or the wiring together of the fragments, holds out any fair prospect of such bony union; that these hooks have not been proved to conduce to this result any more than other methods, nor has the fibrous bond been any shorter, while on the other hand, the most disastrous results have been reported from their use; that when the original separation exceeds one and a half inches, "and especially in * * * re-fracture or rupture of the fibrous bond, accompanied with great separation, it is my opinion that Malgaigne's hooks are entitled to a further trial;" that the plan of Cameron of Glasgow, Rose of London, and others, of opening the joint by Lister's method, and wiring together the fragments, is too dangerous for the "equivalent offered;" that the method of Gould of dividing the quadriceps tendon subcutaneously is not feasible, and the attempt must seriously endanger the joint; that the injection of fresh marrow-cells between the fragments, as recommended by Ollier and others, although safe, "has as yet yielded no results;" that to "produce the shortest possible ligament, and possibly bring about bony union, with safety to the joint," there are two simple indications, viz.: first, relaxation of the quadriceps; and second, approximation of the fragments by pressure.

The first indication is to be met by flexing the thigh upon the

pelvis while the leg is extended upon the thigh, *i. e.*, the whole lower extremity being straight and the foot elevated. The second indication has been fulfilled in numberless ways, but Dr. Hamilton rightly discards the more complicated, and describes his own simple one, which he maintains gives the best results.

The directions for this dressing are as follows: A posterior moulded splint of any convenient material, preferably "shellac-cloth," which has been previously covered by a cotton or woollen sac sufficiently loose to allow the limb to fit into the hollow of the splint, is first made. This splint must embrace the posterior "semi-diameter of the thigh and leg," and extend from above the middle of the former, to within two or three inches of the heel. The splint cover serves as a pad, but is chiefly intended as a "basis to which the bandage * * * may be stitched." A small pad of cotton should then be placed in the popliteal space, and the splint secured to the limb in the following manner: The leg, to within about three inches of the joint, is to be bound to the splint by an unglazed cotton roller. The thigh is then also to be bandaged to the splint with a second roller, the turns not extending lower than about three inches above the joint. "While an assistant approximates the fragments with his fingers, the surgeon makes two or three turns with a third roller around the limb and splint, close above the knee, and an equal number of circular turns are made close below the lower fragment of the patella, and finally a succession of oblique and circular turns are made above and below the fragments, which turns are to approach each other in front until the whole of the patella is covered, the last turns being again circular." The rollers are to be stitched on both sides to the cover, and the foot is to be then elevated some six or eight inches by any convenient means. The author explains his reasons for preferring this dressing, as follows: The unglazed cotton is chosen because slightly elastic and adaptable, sinking in a little above and below the patella, and thus unlikely to slip over the bone. Reverse turns are apt to "spring" and cut. Adhesive strips have been long discarded, as they are apt to slip, excoriate the skin, and cannot be stitched to the cover so as to be held immovably. Pads are apt to be displaced, and are no more effectual than the turns of the roller described. No pad in front of the bone is used, as the last circular turns do away with any such necessity. The skin must be carefully excluded from between the fragments when coaptating

them. No lotions are used, because they render the skin more liable to abrasions.

In two or three days the dressings become loose, owing to the subsidence of the swelling. The oblique turns are then to be tightened by "overstitching them with strong thread" on either side, so far back as to prevent the doubled cloth from pressing upon the exposed portions of the limb. This "stitching" process is to be repeated from time to time when necessary. After the lapse of about four weeks, the whole apparatus must be removed, when the knee will be found quite stiff, and the upper fragment so firmly fixed as to render it unlikely either to be drawn up further by muscular action, or be drawn down by any dressings. The surgeon should then, having the upper fragment gently drawn downward by an assistant, as a precautionary measure, slightly flex the limb, *but without causing any pain or using any degree of force.* The splint should then be reapplied as before, but now its daily removal should be the rule, after which gentle passive motion must be made. Should he so desire it, the patient may now use crutches.

The patient needs no special surgical oversight after the lapse of from six to eight weeks, but when discharged should be warned that for the next three or four months, any sudden flexion of the joint, or, indeed, violent muscular exertion without flexion, may rupture the ligamentous bond. Crutches should therefore be recommended until the dangerous period is passed, with the original splint reduced one-half as to length, fitted with straps and buckled to the limb, as the ordinary knee-caps permit too much flexion. The anchylosis must be left to itself, at least *no force* should be used to overcome it. Dr. Hamilton has discarded his original wooden splint, not because its principle is incorrect, but on account of its weight. He objects to plaster-of-Paris for the same reason, while it also conceals the fragments, and in a few days loses control over them from the shrinkage of the limb.

The treatment recommended for re-fracture is the same as that for the original injury, kept up, however, for a greater length of time. When the fragments are widely separated, Malgaigne's hooks may be used. No retentive apparatus should be applied for more than five months, as then the removal of all support by "teaching the muscles to rely upon themselves," aided by electricity, friction, etc., will prove more conducive to a successful issue.

Our readers will have been enabled by this extended notice of Dr. Hamilton's book, to see that the second conclusion, apparently warranted by the perusal of the literature of fracture of the patella, is not permissible since our means are efficient to fulfil the indications. With regard to the third conclusion, the inference is fair that it is correct since the recorded cases show that good results are attainable by *all* the methods, although the simplest are, *cæteris paribus*, preferable. We think also that the remaining conclusion was warranted, since the profession have certainly, heretofore, not appreciated as thoroughly as they should, the virtual impossibility of obtaining bony union, and that after all a short ligamentous is as good as a bony one.

In conclusion we would heartily recommend this book to our readers, convinced that its careful perusal cannot fail to rectify some errors which are of much too common occurrence. It is made up in an attractive style, is well printed, and has unusually few typographical errors. Everyone who attempts to treat such injuries should see that this work occupies a prominent and accessible place on his book-shelves. [C. B. N.]

Lectures on the Human Eye in its Normal and Pathological Conditions. By ADOLF ALT, M.D., Toronto. With 95 illustrations by the author. New York, G. P. Putnam's Sons, 1880.

Within the past ten years the study of the pathological anatomy of the eye has been largely stimulated by the appearance of the excellent atlases of Pagenstecher and Genth, Becker and Poncet, and by the publication of Graefe and Saemisch's Hand-book of Ophthalmology. These books, however, are very expensive, and not within the reach of all who desire to study so important a subject. Besides, in the atlases, the letter-press is of secondary consideration and merely explanatory of the plates, and in the hand-book mentioned the pathological parts are not grouped together, but are scattered throughout its seven volumes.

The want of a concise and systematic treatise on ocular pathology has been long felt, alike by teachers and students, and Dr. Alt has undertaken and ably discharged the task of supplying what was needed. The method adopted by the author is well suited for the treatment of the subject. The pathological anatomy of the cornea, sclerotic, ocular conjunctiva, iris, ciliary body, cho-

roid, optic nerve, retina, crystalline lens, vitreous body and zonula Zinnii is fully discussed in ten chapters. Each chapter is preceded by a description of the normal structure of the tissue considered, and the author's facts and views on mooted points in ocular histology are clearly stated.

Dr. Alt has produced an attractive, useful, and original book, and we may be allowed to cite from his work some points that are at variance with the researches of other authors.

According to Alt, the ossification of the cornea, described by Stellwag, is probably only a deposit of lime so frequently found in corneal scars.

Pterygium may be caused by marginal ulcer of the cornea in the following way: The swollen and infiltrated conjunctival parts adjoining the ulcer are drawn into it and become attached to its walls and bottom; Bowman's layer is severed from the periphery of the corneal tissue and bent backward upon itself. In a case examined by Alt the conjunctival epithelium, in a state of colloid degeneration, was found between the bottom of the ulcer and the conjunctival tissue of the pterygium. A pterygium is unaltered conjunctival tissue growing like a wedge into the cornea.

In the chapter on epithelioma of the ocular conjunctiva, A. gives the result after his examination of fourteen cases, and says that epithelial tumors of the conjunctiva, whether they start at the corneo-scleral margin, or at a place remote from it, originate in a true hyperplasia of the preëxisting epithelial cells, and furthermore, that the connective tissue, especially the round cells, undoubtedly play an important and active part in their progress. It seems that epithelioma never originates from the epithelium of the cornea proper. The epithelioma usually develops where Bowman's layer joins the conjunctiva and where the normal conjunctival epithelium dips into the underlying tissue in the shape of one or two papillæ.

In regard to the pathology of glaucoma, Dr. A. states, in opposition to Knoes, that he has examined a number of glaucomatous eyes which showed no obliteration of Fontana's spaces.

Our author describes the so-called glioma of the retina as Virchow, Knapp and others do, but prefers to name it a small-celled medullary sarcoma, as Delafield does.

Among the ninety-five wood-engravings we find none that are copied from the works of others, and none that are fanciful and euphemistically styled diagrammatic. All the wood-cuts are copied from nature and accurately drawn by the author.

Dr. Alt's book is a faithful reflection of the present state of our knowledge of ocular pathology, and we recommend the work as a trustworthy guide to those who intend to familiarize themselves speedily with the pathological substratum of the numerous cases, the clinical features of which are so amply illustrated at our various special institutions, [E. G.]

ORIGINAL OBSERVATIONS.

INSOLATION.—COMA.—CONVULSIONS.—TEMPERATURE REACHING 109.5° F. (43° C.).—RECOVERY.

By Dr. F. W. TOWNSEND,

HOUSE PHYSICIAN, NEW YORK HOSPITAL.*

August 24th. Wm. L., æt. 29. American. Porter.

The previous history was obtained from the patient four days after his admission Hard drinker. On the morning of his illness he had been working hard stowing barrels, and late in the afternoon walked uptown to Central Park. He remembered having reached Fiftieth Street, when, being seized with dizziness and darkness before his eyes, he fell unconscious.

At 8 P. M. the patient was brought to the hospital, profoundly comatose. The face was flushed, the breathing rapid and stertorous, pupils contracted, and the pulse very quick and feeble. P. 180, R. 58, T. 109.5° F. (43° C.). A bath was immediately prepared with water at 80° F. (26° C.), and the patient put in it. The temperature of the water was then slowly reduced by adding ice until 65° F. (18° C.) was reached. While in the bath an ice-water enema was given which thoroughly emptied the bowels. The pulse being very feeble, digitaline, $\frac{1}{24}$ gr., with brandy, ℥ 30, was given hypodermically, and a solution of ammonium carbonate by inhalation. The heart responded promptly, and the pulsations became stronger and their frequency reduced. The fever steadily fell, and at 10 P. M. the thermometer registered 101° F. (38.3° C.); then just before removing the patient from the bath a violent tonico-clonic spasm occurred, during which opisthotonos became marked, and was succeeded by grinding of the teeth and

* Service Dr. J. W. McLane.

spasmodic twitchings of the whole body. The seizure lasted twenty seconds.

He was placed on a fever-cot and rubbed dry, but soon had another convulsion resembling the first, though less severe. A few twitchings of the muscles occurred at intervals during the next two hours, but no general seizure. The temperature had meanwhile fallen to 100.8° F. (38.2° C.), but rose to 103° F. (39.5° C.) by midnight; and the patient was wrapped in a wet sheet and sponged with water at 70° F. (21° C.) for three hours. Toward morning there were some signs of returning consciousness, but the fever continued to rise, and it was necessary to stimulate the heart by hypodermic injections of brandy.

25th, 9 A. M. P. 130, T. 104.4° F. (40.2° C.). The patient was still stupid and unable to answer questions, but the pulse was good and his general condition indicated a favorable result. During the day the temperature ranged between 100° F. (37.75° C.) and 101.2° F. (38.4° C.), and in the afternoon the stupor passed off and he swallowed a small quantity of milk.

26th. T. 99.6° F. (37.6° C.) all day. He became fully conscious and complained of headache and weakness.

The patient did well until the evening of the 27th, when he showed symptoms of delirium tremens. He was removed to a cell, treated with bromide of potassium and chloral, and on September 1st was discharged cured.

EDITOR'S NOTE.

Owing to the unusual amount of original matter, the editorial and abstract departments have been crowded out of this number.

E. C. SEGUIN.

Fig 5.

ARCHIVES OF MEDICINE.

Original Articles.

PSYCHO-PHYSIOLOGICAL TRAINING OF AN IDIOTIC EYE.

By EDWARD SEGUIN, M.D.*

This note is intended as the continuation of the monograph on "The Psycho-physiological Training of an Idiotic Hand," which was read last year at the meeting of the British Medical Association, at Cork, and which has since been published in the ARCHIVES OF MEDICINE, Oct., 1879.

We left the teacher of the child deeply impressed with the necessity of continuing the training of his hand with diligence and assiduity. This was done systematically; sometimes by exercises chiefly manual, alternately muscular, articular, or tactile. These exercises were also conducted in view of helping another sense, or another sense helping the operations of the hand; at the same time the hand-training was gradually directed toward the intellection of the sensations obtained by the tact, and the faculty of expressing by the speech the tactile impressions. It is so that there were obtained from the hand of R. many acts of recognition of objects in their totality, and many analyses of their parts; and other acts of resistance, eduction, reception, ejection, prehension; manual operations,

* Read before the Association of Medical Officers of American Institutions for Idiotic and Feeble-minded Persons. Annual meeting held at Barre, Mass., June, 1880.

susceptible of direct employment, or auxiliaries indispensable to other parts of the training.

It is at this point that many educations stop, because in the absence of these rudiments the superstructure has no other guide than automatism; as for R. these exercises prepared him to do intelligently what others do by routine, without thinking, from the beginning, as washing, dressing, holding a pencil, a knife, etc.; so much labor was bestowed upon this part that it was more than a year before his fingers—softer than wax, and relaxed as dead tissues before the rigor mortis has taken possession of them—were capable of buttoning a button, of brushing a coat, etc. Now R. commences to cut wood with his knife with no other object than to strengthen his hand; to cut paper on the traces of the pencil with scissors to give suppleness to his fingers; to trace lines heavy or light, single or combined, on the blackboard; and to throw and receive a ball (yet awkwardly); to give to clay two or three single forms in manipulating it under the pulp of the fingers or rolling it in the hand, and other analogous exercises.

Will it be said that this meagre success is not worth the money and the time spent to obtain it? We think it was for this reason, among others, that this hand was thus allowed to gain, not only in actual capacity, but in eventual capability; not only in direct employment of itself for its own purposes, but in authority as a helper and inducer in the training of the other senses, of the sight for instance, as will be presently seen.

Therefore, passing over the first period, during which the eye, rather more fortuitously than intentionally, helped the hand, we come to the second period in which we prepared the hand to become the leader and often the conscious assistant of the eye in the sensorial training.

But at this point it is necessary to establish the physiological condition of the organ we mean to train.

R., boy, æt. 9 years. Examined Jan. 8, 1880, by Dr. R. W. Amidon. Lateral nystagmus of both eyes. Excursion slight. Some inequality of the pupils. Fundus of eyes seen with difficulty, on account of incessant motion of eyes and head; it seems pale. A hypermetropia of about 3 dioptrics diagnosed.

[Dr. Amidon also made an examination of the blood of R. by Gowers' method with the following results: No. of globules 4,270,000. Normal number of white corpuscles. Red blood globules of good size, and many show process of division.]

NEW YORK, July 5, 1880.

DR. E. SEGUIN :—The idiotic boy whom we examined for you seemed to us to have atrophy of the optic nerves. It was difficult to make a satisfactory ophthalmoscopic examination on account of the nystagmus.

EDWARD T. ELY,
D. B. ST. JOHN ROOSA.

The first observation was taken without preparation; the second, about fifteen minutes after the instillation of a solution of atropine in both eyes. That nothing may be neglected, I note that the child soon gave signs of being under the influence of the drug. He commenced moving his thick lips up and out, not unlike a tapir, saying: "Funny! funny!" and appeared weak on his legs. These symptoms disappeared in about four hours, but there remained a toxic condition of sight which prevented him from threading coarse needles (one of his exercises) for about two weeks. I am to blame for not having these examinations made sooner and oftener; though in such cases the mobility of the organ renders the judgment uncertain.

Referring now to my limited experience, I have observed

in the nystagmus of idiots two alternate periods; one of mobility, the other of fixedness. The mobility consists in a rotary or an oscillating movement, and the fixedness in a a long look side-way-up toward one of the canthi. The latter symptom is more or less accompanied by automatic movements of the hand which pours between the light and the eye a rapid succession of lights and shadows, to the intense, somewhat hypnotic satisfaction of the patient.

The nystagmus of R. was of a kind and degree I had already met with in idiots. Instead of a rotary or oscillating movement, it had a non-rhythmic vibration, and its fixedness had been, but was no more after the first year's training, accompanied with the automatism of the hand just described, which persisted, however, quite independently from the eye trouble. Though great amelioration had taken place in the ocular vibration and fixedness, the former can not always be stopped by the ordinary means of creating attention, nor the latter be directed often, as an intelligent look, toward a meaning objective.

It is hardly necessary to remark—still I note—that, as the weakness of the hand was not its idiocy, so the nystagmus of the eye was not its idiocy; but that the latter, like the former infirmity, was in each case one of the isolating causes added to the isolation of the organ, as this isolation of the organ was added to the general isolation of the idiot. This view is sustained by the facts that in ordinary cases of nystagmus, the intelligence is not impaired to any noted degree; and that in our case, though the nystagmus was only lessened by the treatment, the eye was not prevented by it from carrying many ideas to the cerebrum; and though the hand has not yet recovered all its intended aptitudes, it has served to carry perceptions and objects of perception to the senses, objects to be imaged by the imagination, and objects of judgment to

the *judicium*. This was the main plan of a psycho-physiological training of two years' duration.

But in the meanwhile the physiology of nystagmus had thrown a great light on the treatment of our case. We found that his so-called nystagmus was an automatism akin to chorea and stuttering; an exaggeration of two neurotic incapacities, one of steadying the organs, the other of directing them at will. But due reserve being made for the causes, it does not differ in kind from ordinary unsteadiness of the look more than chorea differs from an inperceptible tremor, nor stuttering from an habitual hesitation of speech. This pathological view of our case transfers it from therapeutics to the physiological training, whose indications to be fulfilled are the incapacity of quieting the eye, the incapacity of direction of the same organ, and the want of intellectuality of the instinctive help the hand affords to the sight.

Thus we emerge from this tedious journey through a maze of incapacities and infirmities of two senses with a clear view of what one of the two needs, and of what the second can do for the first. And, as the hand is the main lever of the training of the eye, let us see what it can do to fix or lead the eye, and what it has done to improve the sight of our pupil.

When the eye is no more capable than the mind of directing itself, the hand can direct it toward objectives in several manners. What language can not do, the hand can, viz., it can present the objects to the eye in their proper light, at the proper distance, at the proper opportunity, and with the proper degree of insistence and pertinacity; even following the eye in its wanderings till it has captured and captivated the regard. The ways of the hand to do this are multiform: it indicates, directs, insists, persists in keeping the eye at bay, or in leading it away from its empty

fixedness. Properly trained by our exercises of imitation, which have become classic, the hand becomes the conveyer of ideas like the fan of the *Senorita* who can, with it, tell anything to anybody. So one can do with a properly educated hand.

But its first use with our pupil is that of *indication.* One can form an idea of the accessory rôle played by the hand in the training of the eye by the name universally given to the finger, *index;* viz., the one which, by pointing out, directs the eye toward objects. It is more effective than force, command, verbal explanation, even imitation; being always practicable unless in the dark; not fatiguing, nor oppressing; on the contrary, gently inciting, always enticing or prompting. Therefore, among the acts early taught to the hand, indication was the object of exceptional efforts, as much to use it with the child, as to make the child use it reciprocally or in relation to objects.

To create thus in the child's eye the habit of looking upon the hand of the teacher, and upon his own, as a leader of other functions, is alone an immense stride in physiological education; yet it is not a process so artificial as may appear *prima facie;* since previous to any education the automatism of the idiotic hand had attracted the attention of the eye, and given pleasure during many empty hours. It is a sufficient proof that the application of the same manual influence in education is not a theoretical device, but one of Nature's own modes of linking phenomena. Thus, imitation and repetition lead to it, analogy extends its use, *besoin* provokes to it, and we intellectualize it by adding a meaning to each indication of the index.

Therefore, though the hand of R. remained a long while, and is yet, in several respects, most incapable, it has been early intellectualized and made to direct the eye. Thus, sometimes his hand, oftener ours, makes him look for, find

out, glance and gaze at, watch, survey, select and follow moving, fixed or ideal objects. The *indicant* hand can do that, and more, though imperfectly at first. Alternately large movements of the hand served to recall the distracted eyes of R. to their centre of useful vision, and small movements of the fingers served to prevent his fugacious look from again starting away. However, these drills of the eyes by means of the hand did not give at once, nor always, what was expected from them. Sometimes insensible, and at other times resistant to the provocations of the hand to look, his eyes would shut themselves or wander beyond reach at the same time that he would point out with his own index in the same direction, and repeat, as if commanding to himself: "Look, look, look." At this point we had to desist.

It is a physiological fact—which has been a puzzle for teachers and an occasion for unjust treatment of children—that several forms of obstinacy, the one just noted among others, are independent of the will; therefore to be avoided sooner than to be affronted, they being susceptible of more intensity and frequency when openly resisted. They are expressions of *inhibition*, a function antipodal to *initiative*. I am not prepared to say, as some do, that the inhibitory process has its special nervous apparatus (Brown-Séquard and others); I am, on the contrary, led by analogy of the mechanism of other functions, of aphasia, for instance, to judge that initiative and inhibition have the same set of motors, which are let go in the act, and refrained in its negative, mainly by excess of tension of the apparatus.

From this view and from experimental results, I conclude that when that negation of initiative has set in, as in the present incident of the training of R., the best to be done is to call the attention of some other organ to some other object, as if the latter were in order. Then, the next time, we shall find that the child has lost nothing by our

yielding, not to one of the devices of his craftiness, but to one of the forms of inhibition which are intrinsic to his synergy.

But "indication with the index" is only one of the means of action of the hand on the eye. There are others, whose description belongs to the classics, which I have hardly room to name, unless they were typical of what was done in our case. For instance, the flabby and impotent condition of the hand—which was the principal object of the training during the first year—required during the second year special exercises, in which the look continued to be the helper, only a better helper as it grew steadier; and conversely, the eye was helped the more by the hand, as the latter became more serviceable, so that at the middle point where the two series of exercises met, it would have been impossible to tell which was the main object of the training, the hand by the eye or the eye by the hand; both being teamed for the same onward course. The philosophy of this progression will come out from the description of two or three of these exercises, as space will allow.

We might consider as typical of the exercises in which the hand leads the eye—though the reverse seems true to a superficial observer—the retracing by the child of a figure already traced in different colors by his teacher. Here the hand is directly obliged to follow a track; but in order to follow it, must call upon it, by a reflex action, the attention of the eye. It is thus that both hand and eye become, one directly, the other reflectively, fixed on the same point, or mobilized on the same course. This exercise is part of a series, and about midway in that series of those instituted, either to bring to its centre of vision a side-way-up regard, or to attach a vacant look to an immobile object, or to move an immobile regard along with a mobile object, existing or ideal.

One of the sequels of this plan, in which the hand becomes more and more engaged, and the eye more and more precise, would be to closely prick with a stiletto the same lines traced on paper; to cut with scissors the same lines previously traced by the master, retraced and pricked by the pupil; and analogous exercises tending directly or reflectively to act on the leaders of vision; and by accustomance, to alleviate or cure the disorders of function which have kept the eye, from its normal communications with its *milieu*, isolated (*idios*),—an idiotic eye.

Hence, continuing the series by which the child has been initiated to "drawing," without knowing that he "drew," we came to "modelling." This primary creative exercise of the hand is a good representative of the coöperation at different and variable degrees of several senses, for the accomplishment of a common act; and one which proved to be particularly well adapted to meet some of the defects of the hand of our subject. But when we wanted to begin, it became manifest that the hand which had been taught to support the weight of the suspended body, of dumb-bells, and the shock of the balancer, and which rendered many personal services, was yet too soft to leave an impress on soft clay; and was yet as awkward at willed-pressure by relaxation, as others are made by contracture short of actual paralysis. Then we had to teach hand-pressure separately, with objects like the rubber ball, whose material alternately yielding and reacting, communicates to the hand the same ebb and flow movement.

This R. had to learn by mere obedience, enforced by a constant surveillance—it being without an appreciable object, and tiresome—till his hand began to react against the reaction of matter, and became able in its turn to impress his plastic imprint on it. Then he was given a lump of clay to work upon. But at first his work amounted to this: The

fingers being, as it were, scattered by their weakness—that is, incapable of coöperating together—could not embrace effectually the lump, which, fitting better in the palm of the hand, began to be rolled therein. The theorist would say that a ball would come out from this rolling, and the kindergarten teacher would invite the child to make one; but what with the reality? He could not. What comes out from his hands is not a ball, but a cylinder, inequally tapering at both extremities to suit the shape of the two halves of the mould. And why? Because not only the eye had no part in the shaping of the clay, but the tact neither. The cylinder is the product of the sense of muscular activity, but the ball will result from the combination of this sense with the tact to produce the double movement of rotary pressure, equally necessary to make the lump of clay become a ball in the hand of the idiot and to model the spheres above. Happily the tactile sense of our pupil had been previously trained with great care, and having never presented anomalies, responded soon to the calls made upon it for the the control of nascent elementary forms, with or without the help of the look.

But to form a cube, a pyramid, etc., by hand-moulding is a different affair, for which the hand alone, even with its two senses, does not suffice; the eye is called to interfere. Here another and graver error of method lies in ambush. R., clay in hand, is told by a teacher, who was volunteering her kindergarten experience for the occasion, to strike the soft mass gently on the table, flattening now one side, then another, till the six surfaces of the figure had become plane as the table itself. This may be a good means of making a cube. The object of the kindergartner being to make a cube, she was right; but it was not our object. We aimed not at cube-making, but at exercising the power of "impressing an idea of form upon matter" by an infinite variety

of pressions of the neuro-muscular pulp concentrated in the finger-tips.

Had R. been trained for a show, the making of a cube, or of any other figure, with his hand as a holder, with the table as a moulder of even surfaces, and with the two branches of a compass measuring instead of the eyes, would have been justifiable. But making it as a means of training the senses, all that was entrusted to the table, compass, or any mechanical helper, was so much stolen from the physiological training of the hand and eyes, at the very time this sensorial training was the acme of the curriculum, and when object-making was only one of the means toward that end ; at the time, too, when the hand had to help the eye, and the eye the hand, till, by an insensible gradation, the eye would soon be able to act independently. True, the kindergarten expedient gives more showy results, and is superior to classical recitations ; but it is wholly unphysiological as a method, and gives premiums, also mortgages, to automatism.

Having explained how the hand of R. was exceptionally unfitted for aggressive work, and his eye for steady look, I am prepared to acknowledge that the combined exercises of these two senses did not give very startling combined results. In drawing, where the hand does not need to emphasize its acts with force, he showed pretty good results. In modelling, commenced quite recently, the hand has produced nothing worth showing, but we do not stand by that test ; nay, I deny the test of object-making, and searching for the physiological results, I obtained the following : (*a*) The hand, though yet incapable by itself of leaving a marked and useful impression on matter, has gained a neat consciousness of what it could do, if it would do ; and of what it is capable under the leadership of another's will. (*b*) The eye is infinitely more responsive to the indications, directions,

presentations, provocations of the hand than it used to be; the vibration of the eye is smaller, and the fixedness in a canthus less frequent, certainly less obstinate; and the organ and sense of vision are altogether more obedient to the appeals of the mind, or of others' mind. This sense is kept busy two hours and a half under my husbanding, and with less tension the rest of the day by his teacher.

I entitled this paper *Psycho-physiological training of an idiotic eye*, and now I see before and behind me two objections to that title: one, to the justice of which I am sensible; the other, whose injustice makes me smile.

The first and just objection is that, after promising so much in the title, I have not been able to enter my subject farther than the point where and how the hand helps the eye to look. There is no denying the fact. But was it not better to be thorough in the preambles than to ramble in the vastness of the subject? I will only sketch here what I would like to give later in topographic sections, carefully delineated, time permitting.

A moment of reflection will give the link which leads from taking, as by the hand, the eye of an idiot, or of any other child, out of its socket, to making it look by others' sensory provocations, or by gradual directions of the mind. In this work we found the demonstration that the mind, which has not been stocked with the treasures of sensation, cannot return the gems of intelligence. This rule is probably more strict for idiots, who get less of informal education, than for ordinary children, who learn something from anything or anybody, by induction or by deduction, *per fas aut nefas*. On the other hand, there is great disadvantage for the idiot who, for a long time, can learn only what he is taught methodically; but a great advantage too, because there is an unsurpassed realism in what he succeeds in acquiring by the physiological method; whence results for him a

rectitude of conception, which is rarely the attribute of children stocked from all kinds of sources. Therefore, I cease to describe, in order to trace the source whence came the present stock of ideas of my pupil.

It is a great day in intellectual life when the eye can direct itself toward an object, fix itself on another, and pass, from one to another, the two branches of its compass of comparison; but it is a rarer event that this opening be used pathologically and mathematically in the education of many children, and it is due to idiots, as far as I know, that this theory has ever become substantiated in education.

In substance it is this: When the eye begins to be responsive to the appeals of the other senses, if every perceptible object has been presented—and perceived—in apposition to its opposite, the impression of the difference manifested between the two is impressed. If every object is also presented by the face of its most evident properties and uses, and these properties and uses likewise perceived; and if we have done the same for the other elements of things, as forms, dimensions, proportions, combinations of parts, etc., we have not only accumulated images ready to be evoked at some future call for the wants of imagination, but have given rise, between two or more of these images, to comparisons, absolute creations of the mind, entities which can be evoked in their turn at the call or by the force of reason, passion, etc.

From this process of creating ideas to the inverse process of realizing or embodying ideas, there is only a question of handicraft. How far does it go? It depends on the care bestowed on the senses, and their natural dispositions or impediments. This sensorial method of training was used for R., and though it had to battle with the extraordinary pathological difficulties detailed above—the native incapacity of the hand, and the nystagmus of the eye—it gave,

during the second year of his education just ending, most satisfactory results. To quote only those derived from the education of the senses: his hand begins to be capable of some exercises of aggression, is decidedly capable of several forms of resistance, and quite skilful at a variety of tactile recognitions. The vibration of his eye has diminished, his intentional look is more steady and his automatic one less riveted.

From things and acts to words, the gradation was this: At first he could but repeat the last word of what was said to him interrogatively or otherwise. To create his vocabulary, which is now pretty accurate, we identified the word to the thing; we not only named the objects, their qualities, movements and modalities, but presented to him the words as we had the objects—that is, grouped by pairs of contrasting meanings, or in clusters of analogues, never telling him one thing without opposing to it the contrasting idea, or some analogous idea, till his countenance showed that he had a glimpse at the idea glimmering before his mind, as once glimmered before his vacant eye the lights and shadows passing through his fingers agitated by automatism. The same for pictures and portraits, of which he is very fond, because he has been taught to attach a meaning to every delineation. Thus he knows not a few great or good men by their deeds and faces. When he speaks, in his yet confused language and articulation, it is with an inexpressible delight that I hear him not only find the word, but even express other ideas by implication, as he did this morning (May 22, 1880). It was very hot. Seeing him on a lounge, deliciously lazy, I said: "Well, R., what can I do to rouse you? Must I buy a big whip? What shall I do?" "Play ball with us," was his ready answer.

Three months ago he could not catch the ball, now plays it with accompaniment of a humming song. I give this

episode of our daily life as a pleasant average of our habits, and as a key to the psycho-physiological method of training, which, starting from the lower-than-animal movements, imitation or obedience, rises up to the infinite delicacies of comparing forms, dimensions, words, ideas, faces, moral expressions, and of participating in moral feelings.

To come to the second objection which might be made to this paper, I had better meet it half-way, since it has been already muttered against the preceding one. Some objected to an idiotic hand, as they may object to an idiotic eye, on the plea that idiocy is the privilege and monopoly of the head. This *Kephalism*, or Cæsarism of the cephalic centres has been investigated and found insufficient. The latest physiologists have studied the functions of the great sympathetic as a regulator of caloric, and its relations to the millions of peripheric nerves which form with it, quite independently of the brain, the demo-neurotic apparatus of the life of relation. This was the object of the last experiments and thoughts of Claude Bernard. Our physiological training of idiots may be considered as a long series of experiments (from 1838 to 1880) tending to the same conclusions. Now, a theory and practice of the education of idiots without room for the sympathetic, nor for the peripheric forms of idiocy, and pretending to improve idiots by teaching ideas to their brains, would not gather more adherents than the doctrine which attributes to the king the power of curing scrofula or insuring abundant harvests, etc.

But let us suppose that this new physiological theory be false and replaced by another; this fate has eventually overtaken the preceding theories without affecting their value and position in the link of science. A philosophical mind does not accept them as absolute but as relative truth, doing duty in their place and time, as this new theory of psycho-physiological decentralization will do in helping to solve

separately the problems insoluble by the cephalic theory. Likewise, as the problem of idiocy and its treatment was once declared insoluble from the stand-point of the cephalic unity, it may be considered as solved from the stand-point of the hypothesis of a poly-energy, which attributes to each apparel its own energy, and to the whole a synergy proportionate to the power of reflex conduction of the sympathetic.

Now, two things are necessary to confirm this hypothesis: 1. More histological researches, in which the physician for idiots must take a leading part. 2. A more searching analysis of the psycho-physiological processes by which sensations are converted into images, images into ideals, ideals into objectives, objectives into realizations; closing the circuit between mind and matter, so-called, by the proof that the circle is not made up from a diversity of substance, but of functions and operations.

Therefore, I conclude that, as to help solving the anatomical problem, most of us need the concourse of men skilled in the arts of modern observation; so, to advance the psycho-physiological question, we need give a larger scope to the mind of our teachers. On every side the same want is felt. The nurses demanded by the physician and the surgeon must be trained, so that their reports may be trusted. Likewise, our teachers must be more especially educated in the art of analyzing the psycho-physiological functions of their charges, in order to give us reports whose reality can serve as a solid basis to our generalizations. Without these reports we can improve idiots, but we cannot advance our art nor furnish to anthropology the contingent expected from us. If the present sketch, as well as the preceding one, has any value, this value comes from the realism of the teacher's observations which we have only idealized by the truthful process of generalization.

EDITOR'S NOTE.

It is the Editor's painful duty to announce that the above article is published posthumously. Dr. Seguin died on October 28th, just as he was fairly entering upon a new development of his life-work, viz., the education of feeble-minded children.

We believe, with several better judges than ourselves, that this article, together with the one which appeared in October, 1869, contains the enunciation of principles which will mark an epoch in the education of normal children. The central idea underlying these principles is that the peripheral organs, the special senses, and the cutaneous and muscular senses must be carefully trained or educated, at the same time that the more purely cerebral functions are developed by ordinary teaching. This great physio-psychological conception must, in time—perhaps a long time—attract the attention of teachers in kindergartens, schools and colleges.

Explanation of the plate.

Lithograph made from a photograph of R., the pupil mentioned in the essay. This view shows his expression as improved by physiological education up to the spring of 1880.

In the ARCHIVES OF MEDICINE, October, 1879, was a plate containing four figures, illustrating the life of the same pupil from babyhood to 1879.

THE DIAGNOSIS AND TREATMENT OF OBSCURE PELVIC ABSCESS IN WOMEN, WITH REMARKS ON THE DIFFERENTIAL DIAGNOSIS BETWEEN PELVIC PERITONITIS AND PELVIC CELLULITIS.

By PAUL F. MUNDÉ, M.D.

I SHALL begin this paper by a review of the differential points, anatomically and clinically, between inflammation involving only the pelvic peritoneum, and inflammation confined to the pelvic cellular tissue. I am induced to bring up this old, much-discussed question again, by a recent revival of the controversy as to which of these two affections is the more frequent, and the denial, by a prominent French gynecologist, that such a thing as cellulitis of the broad ligaments exists.

Filling all the interstices and cavities underneath the serous membrane which separates the abdominal from the pelvic organs, is a loose network of cellular or areolar tissue. This serous or peritoneal lining membrane of the upper portion of the pelvic cavity, passes from the anterior abdominal wall to the vertex of the bladder, thence dips slightly into the space between uterus and bladder, forming but a shallow pouch, encases the fundus and body of the uterus in the median line, and passes down behind the uterus almost to the level of the external os, forming a very deep, the well-known Douglas' pouch. This pouch is bounded on either side by two loose folds of peritoneum,

the utero-rectal ligaments. Laterally, the anterior and posterior layers of peritoneum come quite close together at their upper border, having the Fallopian tube between them, and form the broad ligaments of the uterus; as the two layers dip down into the pelvic cavity they separate, the anterior being reflected upon the anterior abdominal wall, the posterior descending to a lesser depth than in the centre, and then passing up on the anterior wall of the rectum. Laterally, the two folds of peritoneum flatten out, as it were, and pass up on the lateral pelvic wall and clothe the iliac fossa. The retro-lateral peritoneal pouches are, therefore, less deep than the central, Douglas' pouch, which latter has in rare instances been found to descend in the recto-vaginal septum almost to the perineum.

Occupying the space between the peritoneum and vaginal roof behind and on each side of the cervix, between the layers of the broad ligaments, and at the upper portion of the subperitoneal space between bladder and uterus, is situated the cellular or connective tissue with its numerous blood-vessels and lymphatics on their way to and from the uterus. The presence of this cellular tissue between the layers of peritoneum forming the broad ligaments, has recently been denied by Guérin, who asserted that he was unable to find any such connective tissue between those layers; he therefore corroborated the old statement of Bernutz, that the evidences of inflammation and plastic exudation found in those localities, were due to pelvic *peritonitis* and not pelvic cellulitis; that the serous membrane was inflamed, not the cellular or connective tissue underneath it. Bernutz, indeed, claimed that pelvic cellulitis is a rare affection, but pelvic peritonitis a very common one. Investigations by König and Schlesinger have demonstrated the existence of an abundance of highly organized connective tissue between the layers of the broad ligament and all

about the cervical portion of the uterus, and König's researches having been made both at the bedside and the post-mortem table, have shown particularly the great inflammability of this tissue, and the rapid spread of inflammation from one section to the other. When Guérin, therefore, denied the presence of connective tissue between the layers of the broad ligaments, and asserted that for this reason there could be no such affection as phlegmon, cellulitis and abscess of the broad ligaments, the swelling and tumor felt laterally to the uterus being intraperitoneal and due to pelvic peritonitis, I felt from my clinical experience that he must be mistaken. Wishing to hear the opinions of other competent observers on this point, I brought this subject before the New York Obstetrical Society at a recent meeting, and was met by a unanimous response confirming my views. Dr. John Byrne, of Brooklyn, even informed me that, a number of years ago, after Bernutz' and Goupil's statements appeared, wishing to test this question as to the existence, extent and communication of the subperitoneal pelvic connective tissue, he introduced a blowpipe under the pelvic peritoneum and injected air, with the result of distending not only the broad ligaments, but lifting the whole peritoneal lining of the pelvic cavity.

Spiegelberg says in a clinical lecture that the whole cervical portion of the uterus,—that is, that portion of the cervix situated between the reflection of the vagina and the internal os,—on all sides except in front, is surrounded by abundant loose connective tissue, and that the retro-uterine peritoneum is not closely attached to the uterus. Of this I have had occasion to satisfy myself while testing the possibility of enucleating a uterus from its peritoneal envelope through the vagina, as would be indicated in carcinoma of that organ. I found that the peritoneum could very easily be detached from the uterus up to the os internum, but

that above it was impossible to separate it except by actual dissection with the knife. In front, this detachment was by far less easy than behind and at the sides, owing to the small amount and density of the connective tissue which, at a spot corresponding to the os internum, is interspersed with muscular fibres, and assumes the character of a ligament, whereby the uterus and the bladder are so closely connected as to be almost inseparable. This abundant paracervical connective tissue permits the occurrence of a paracervicitis and the formation of swellings of plastic lymph at either side of and behind the cervix, which need not extend farther up, and are generally small in extent.

Such an extraperitoneal plastic exudation is, therefore, possible behind, as well as to either side of the cervix, and can be distinguished from an intraperitoneal effusion into Douglas' pouch by its smaller size, the rapidity with which it solidifies, and its lower position behind the cervix. I have seen such a retro-uterine paracervical exudation extend halfway down the recto-vaginal septum and prolapse the posterior vaginal wall like a rectocele. Ante-uterine parametritis or cellulitis is rare, for the reason of the comparative dearth of connective tissue in that region.

The place of election for inflammation of the pelvic connective tissue is between the layers of the broad ligaments, where the profusion of blood-vessels and lymphatics running to and from the uterus, the presence of the ovaries, with their frequent periodical congestion and irritation, and the loose character of the meshes, particularly where the peritoneum spreads out antero-posteriorly and laterally over the roof of the vagina, afford most favorable opportunities for inflammatory action and plastic effusion. This antero-postero-lateral expansion of the peritoneum, like the wings of a bird, permits the serous and plastic effusion, by which an inflammation of the connective tissue first demonstrates

itself to the touch, to spread out into a large tumor, which may entirely fill one-half of the pelvic cavity, extending anteriorly and laterally to the ileo-peritoneal line at the pelvic brim, and posteriorly to the sacro-ischiatic notch; toward the median line the exudation may spread in the retro-cervical cellular tissue to and beyond the cervix, and be felt as a hard, more or less pointed mass immediately behind the cervix. One-half of the vaginal roof is then bulged down by the exudation, and assumes a convex appearance. If the exudation is large, it may crowd the uterus entirely to the other side of the pelvis.

How, now, can we differentiate between a pelvic cellulitis, —an inflammation and exudation into the cellular tissue of one broad ligament and one-half of the pelvic cavity, and a pelvic peritonitis,—an inflammation of the peritoneal envelope more or less covering that same pelvic cellular tissue, which also shows itself by effusion and localized swelling?

In the first place, I believe that the two affections are so often associated, as indeed their proximity and vascular connection would lead one to expect, that it is simply impossible to differentiate between them; with an exudation into the supravaginal connective tissue, we always have a plastic effusion on the surface of the contiguous peritoneum or into the adjacent serous pouch.

This is the opinion expressed by Emmet in his recent book, where he confesses his inability ever to distinguish between perimetritis and parametritis at the bedside, and pronounces the distinction to be merely a theoretical one. In this I cannot help thinking he goes too far, for there are doubtless typical cases in which the two affections can be separated, and I have met with many cases in which I was sure that the, to my mind, rarer pelvic peritonitis existed alone, and many more instances in which I am confident the connective tissue alone was involved. The etiology of the

affection may, to a certain extent, aid the diagnosis; thus, irritation proceeding from the fundus uteri and the tubes (salpingitis, fundal endometritis) will be more likely to produce peritonitis; injuries of the lower portion of the uterus, cervix, internal os (cauterization, discission, sounding, dilatation, impetuous coition), on the other hand, result in inflammation of the adjacent cellular tissue. The same may be said of the ovaries, which are embedded in the connective tissue of the broad ligaments. These distinctions, however, by no means hold good in all cases, for we not unfrequently find distinct cellulitis following an intra-uterine application, and peritonitis may ensue after dilatation of the uterine canal.

Thus, I have during the past summer met with three cases of undoubted, distinct, intraperitoneal effusion, all directly behind the uterus, in Douglas' pouch, the cause (so far as ascertainable) being in the first case a protracted digital and bimanual examination and the repeated passage of the sound; in the second, an exposure to cold and surf-bathing; in the third, the too long retention, in spite of positive directions, of a smooth, hard-rubber stem pessary.

Quite recently I saw a cellulitic exudation of the size of half an orange follow the application of the solid stick of nitrate of silver to a cervico-vaginal fistula remaining after an otherwise successful operation for laceration of the cervix; and I have seen two instances in which a very large cellulitic exudation followed too violent coition, and several in which the pressure of an ill-fitting or displaced pessary produced circumscribed exudation at the point of pressure. In all of these cases there certainly was no combination of cellulitis and peritonitis, especially not in those exudations due to pressure of the pessary, which were too low down in the pelvis to have any possible connection with the peritoneum.

The physical shock is a better criterion, for that of a large

intraperitoneal effusion is vastly more severe than from an exudation into the connective tissue, which latter is often scarcely attended with fever or pain. Still a very large cellulitic exudation, such as I have seen extending almost to the vulva and in the retro-peritoneal cellular tissue up to the crest of the ilium, will produce as severe collapse as any intraperitoneal effusion of serum.

The physical signs as elicited by the touch give the best diagnostic evidence. In uncomplicated pelvic peritonitis the finger finds the vaginal roof thickened, as though it had been coated with water-glass or plaster of Paris; but if there is no large fluid effusion, it is not flattened or bulged downward, but retains to a great extent its arched form; it is merely stiffened, as it were. If there is fluid effusion, however, there will be a certain amount of bulging downward, and, so long as the fluid is not encapsulated, there will be a diffuse doughy feeling in the retro-uterine space. But this lasts but a very short time; the fluid becomes encapsulated by the matting together of the intestines above, and we now have a distinct retro-uterine tumor reaching down to the depth of Douglas' pouch, on, or nearly on, a level with the external os, and in proportion to its size pushing the uterus forward. This tumor may either fluctuate, or, as coagulation usually occurs very soon, it becomes doughy and ultimately board-hard. This exudation of pelvic peritonitis is almost always directly behind the uterus, because the fluid effusion naturally sinks to the lowest level of the peritoneal pouch, which is in the median line. Only when the effusion is so large as to extend into the retro-lateral pouches, or when the effusion occurs outside of the utero-rectal ligaments and is small in amount, will the tumor be felt laterally, or as far as the pelvic wall, and it is always situated in the posterior half of the pelvis, *behind* the broad ligament. This is a very important distinction, for an exudation *into*

the broad ligament, between its layers, while it may extend backward to the pelvic wall if large enough, will also extend in the opposite direction anteriorly. Besides, exudation into the broad ligament always pushes the uterus to the other side; a retro-uterine intraperitoneal effusion, however, pushes the uterus forward. Only when the cellular exudation is very large and extends down the recto-vaginal septum will it push the uterus forward; but this same downward extension then settles the diagnosis. The small exudation-tumors of paracervical cellulitis cannot be mistaken for anything else; their low position, close to the cervix, sometimes encircling it like a collar, their usually comparatively small size, and their history and want of violent constitutional symptoms will generally render their correct diagnosis an easy matter.

As a rule, it may be held that in *pelvic peritonitis* the only distinct tumor formed is behind the uterus in Douglas' pouch; that this tumor pushes the uterus forward, if sufficiently large; that it does not reach down to a level with the external os, and that in pelvic peritonitis the vaginal roof is but rarely depressed by an intraperitoneal effusion. If a lateral tumor is felt, it is only when the effusion has become so large as to exceed the capacity of the median pouch, and this lateral tumor is then always posterior to the uterus. The tumor can be felt by bimanual palpation only when very large.

In *pelvic cellulitis* the tumor is usually felt at the side of the uterus, may reach behind and below the cervix if the exudation was large enough, but then is usually as low as the external os; laterally it bulges out the anterior vaginal wall, and may also fill the whole posterior segment of the pelvic cavity; it is felt at once as soon as the inflammatory symptoms appear; and the exudation, being confined by its surroundings, must always form a tumor. It always pushes

the uterus to the opposite side, and not forward, and can usually be felt without difficulty by bimanual palpation.

In both affections the rigid exudation renders the uterus immovable. Another point in diagnosis has appeared to me valuable, and that is, that in considerable but diffuse intraperitoneal effusion the whole vaginal vault and uterus are more or less pushed down, prolapsed, and become fixed when the exudation solidifies; in cellulitis, however, the uterine displacement is always a lateral one, never downward.

A few days after this paper was written and read at a private medical reunion (on June 16, 1880), the June number of the *Edinburgh Medical Journal* came into my hands with the regular exchanges of the *Journal of Obstetrics*, and in it I found an article by Dr. Angus Macdonald, President of the Edinburgh Obstetrical Society, on "Three Cases of Parametritis, with Observations on its Diagnosis and Treatment." In this paper, Dr. Macdonald discusses with his usual ability almost precisely the same points in the differential diagnosis between *para*metritis and *peri*metritis, and gives substantially the same distinctive features as these just described by me. It was gratifying to me that so acute an observer as Dr. Macdonald agreed with me in this matter, and laid down almost the same distinctive features between the two lesions as I had found myself. That the diagnosis is usually difficult cannot be denied, but that such a distinction exists clinically, as well as theoretically, and can be recognized by the signs mentioned, seems to me equally certain, although it may seem like presumption to say so in the face of Dr. Emmet's opinion to the contrary.

Thus, in a case of undoubted pelvic peritonitis following the too long retention of a stem pessary (to be referred to later on), at the first and second visits the retro-uterine tumor was smooth, globular, situated on a level with the posterior

insertion of the vagina into the cervix. At the third visit, however, one week later, the lower outline of the tumor was deeper, irregular, reaching to the level of the external os, and extending more bilaterally. Evidently a later exudation had taken place into the retro-uterine cellular tissue, and the case had now changed from one of pure pelvic peritonitis, to one of combined pelvic peritonitis and cellulitis.

The practical importance of this distinction between pelvic peritonitis and pelvic cellulitis as regards prognosis, greatly exceeds that as regards treatment. An intraperitoneal effusion is always a more serious matter, and is more liable to leave traces behind it in the shape of adhesions and obscure abscesses than a cellulitis. The prognosis, therefore, should be more guarded in peritonitis, although the majority of cases recover. As regards treatment, both affections will be amenable to the same measures, and will show equal tardiness in recovering.

There are so many different opinions as to the occurrence and relative frequency of pelvic peritonitis and pelvic cellulitis,—some (Bernutz and Goupil, Aran, Courty, Guérin) claiming that pelvic peritonitis is by far the most common, others (Nonat, West, Hewitt, Emmet) giving the preference to cellulitis, and others, again (Virchow, Duncan), merely admitting the occurrence of both varieties without distinct expression of their relative frequency,—that any competent observer may be pardoned for adding his own experience. I certainly do not agree with those authors who, like Aran and Courty, deny the cellulitic character of all large intrapelvic effusions. I have already stated that I have seen a case where the posterior vaginal wall was prolapsed like a rectocele by the enormous plastic effusion in the cellular tissue of the recto-vaginal septum and underneath the pelvic peritoneum, the effusion reaching up be-

hind the peritoneum on the left iliac fossa as far as the crista ilii, and down nearly to the perineum. Besides, I have often seen cases where one-half of the pelvic cavity was occupied by a solid plastic exudation, the low position, peculiar situation, and contiguity to the pelvic wall of which, rendered its cellulitic nature unquestionable. Intraperitoneal effusions may appear and be very large, especially when the intestines are matted together by the plastic exudation, and form an apparently solid mass; but I have seen them merely filling Douglas' pouch and not extending laterally at all.

If an effusion is more readily palpable through the abdominal wall than it is touchable by the finger in the vagina it is generally intraperitoneal. Only when a cellulitis is anteperitoneal (in front of the uterus or under the sheath of the abdominal muscles) or retro-peritoneal (extending up into the iliac fossa), is the tumor apparent and readily palpable by the external hand alone. I recently saw a case in which the exudation was evidently only in the upper portion of the left broad ligament, probably encircled the ovary as a nucleus, and not extending downward into the pelvic cavity, was distinctly recognizable only by conjoined manipulation. Diffuse parovarian infiltrations in the broad ligaments are very common, but I have found a distinct ovoid exudation about the ovary and movable by bimanual examination with the ligament rather rare. This point is of interest and importance as regards diagnosis, for such a faintly-movable spherical or ovoid tumor in the upper portion of the broad ligament might readily be mistaken for an ovarian tumor or pediculated uterine fibroid. The history (chills, fever, short local pains, vesical irritation, etc.), the peculiar hard, knotty feel, irregular outline, and, after all, merely apparent mobility of the growth (which can be moved only by pressing the whole ligament

up and down between the two hands in conjoined manipulation), will ordinarily prevent an error.

As regards the relative frequency of the intraperitoneal and cellular effusions, I find that among something over 2,000 gynecological cases of which I have accurate and full notes, there were 108 cases of distinct well-marked pelvic effusions; of these 73 are noted as suffering from pelvic cellulitis, the precise seat and extent of the exudation being given in all; and 26 were pronounced to be pelvic peritonitis, on what appeared to be good and sufficient evidence. Of the cellulitis cases, 33 are noted as being chiefly on the left side, 21 on the right, 9 in both broad ligaments, and 10 behind and surrounding the uterus.

Of course, the correctness of the diagnosis may be questioned, but I think that an adherence to the distinctive points mentioned above prevented errors. The cases were all examined repeatedly at intervals, and the previous diagnosis thus controlled. In 9 cases distinct coexistence of cellulitis and peritonitis was noted.

To explain the rarity of the combination which, according to the general idea and my own opinion, is the rule in inflammations involving large tracts of pelvic tissues adjoining the peritoneum, I should say that the diagnosis was always made as that of the affection the evidence of which was the most prominent in the case. The cases in which the signs of cellulitis and peritonitis together were sufficiently clear and distinct to leave their coexistence undoubted, were thus rare. I have no doubt whatever that a slight film of plastic exudation on the enveloping peritoneum accompanies every case of cellular effusion; but this film is ordinarily not to be detected by the touch or general symptoms. Only such cases, therefore, in which there was undoubted evidence of both varieties of inflammation were noted as such.

I have not included in the above list of cellulo-peritonitis the undoubtedly very much larger number of cases in which uterine displacement, ovarian congestion, subinvolution, areolar hyperplasia, are accompanied by slight, often entirely unrecognizable effusion into the cellular tissue, or thickening and shortening of the uterine ligaments. Emmet places the proportion of these generally overlooked cases as high as 80 per cent. My attention has only recently been called to this etiological factor of uterine disease, and I am, therefore, not able to give figures as to its frequency.

I will now proceed to speak of the diagnosis and treatment of a peculiar variety of pelvic abscess which I have met with several times during the past two years. By pelvic abscess I mean an abscess formed as the result of, and in an exudation of plastic lymph in the cellular tissue of the pelvis, and in this paper I wish to refer solely to abscess of the broad ligament, in which locality all the deposits here referred to were situated. I have described above the exudation-tumor occupying a large portion of, or the entire half of the pelvic cavity, which is the typical example of a pelvic cellulitis. Its feel is familiar to all of us, and once acquired cannot easily be mistaken. This exudation into the broad ligament (generally the left one) is bounded behind, above, and before, by peritoneum, laterally by the uterus and pelvic wall, and below by the vaginal roof. Between the exudation and the finger is, therefore, only the wall of the vagina.

An exudation of this kind is ordinarily soft, doughy during the first few days; but it soon hardens and loses the tenderness which it at first had to a high degree. After a while, as the process of absorption of the fluid elements of the exudation proceeds, the mass shrinks and becomes as hard as a board. Under the usual treatment of blisters or leeches (only in the very first stage), poultices, continued for

weeks, as long as there is the least tenderness or the exudation has not reached the apparent limit of shrinkage, hot injections, inunctions of iodide of potash, and mercurial ointment, the proto-iodide of mercury, or chloride of gold and sodium, tonics, etc., in the majority of cases the exudation melts away little by little, usually occupying two to eight weeks in this process, and is either entirely absorbed, or, what is more common, the patient leaves her bed with a life-long memento of cicatricial induration in her side, with her uterus more or less displaced by adhesions, and the ovary of that side encased in a shell of organized lymph and unfit for further duty. It has been my fortune to have seen a large number of cases of pelvic cellulitis, not of my own manufacture, be it understood, although perhaps I may plead guilty to a few. Of all these cases, doubtless exceeding several hundred, for seldom a week passes now without one or two cases coming under my notice, only a very few have run a different course and terminated otherwise than above mentioned. In a few the exudation has gone on to suppuration, and the abscess has discharged through the rectum (most frequently), the vagina (three times), the bladder, and the abdominal wall (each once). In the other few still remaining to be described, seven in number, the disease took a different course and another method of treatment was finally employed.

In all these seven patients the remedies above described had failed to dissipate the exudation, although faithfully and persistently employed for months. The patients all came under my care after they had been under other medical treatment. The exudation in all the cases possessed the characteristics described above: a large solid tumor to the side of the uterus, filling up one-half of the pelvic cavity and reaching deep down to the pelvic floor, and distinctly palpable by bimanual examination. In four cases the tumor

was on the left, three times on the right side. The tumor was not at all or slightly tender to the touch. The date given as that of the first attack of inflammation varied from three months to two years. The symptoms complained of by the patients were due to the local disturbance from the tumor (pain in the side and back, bearing down), besides poor appetite, debility, anæmia. In one case distinct chills had occurred at irregular intervals for several weeks, and the patient had a decidedly cachectic appearance, with varied temperature and pulse. The size and rounded contour of the exudation, its refusal to become absorbed after so long a duration, and the cachectic appearance of the patient, led to the supposition that there must be some unsuspected cause for the persistence of the exudation and the constitutional symptoms. And what supposition was more plausible than that a retention and consequent slow absorption of pus was the cause of the trouble? That this was the case with the patient with chills was more than probable; but in her case, as little as in the others, was it possible to detect even the slightest trace of fluctuation by direct and bimanual palpation. The mass was firm, solid, although not exactly hard, and at no spot was there anything like pointing. But a peculiar doughy feeling was imparted to the internal finger; a sensation similar to that repeatedly felt when an exploratory incision subsequently revealed very deep-seated parenchymatous suppuration, as after erysipelas and gunshot wounds; and there were several softer spots which were easily indented by the finger.

Case 1.—Although there was absolutely nothing positively indicating fluid or pus in the first case of this kind, which occurred to me about two years ago, except the persistence of the trouble and the rounded contour of the mass, I decided to aspirate, and take the chances of finding something or of increasing the exudation. I therefore carefully felt for a spot where the

mass seemed at least a trifle softer, and, under guidance of my finger, passed an ordinary hypodermic needle into the vagina and plunged it into the mass up to its hilt, a proceeding very much interfered with by the shortness of the needle and the syringe. On withdrawing the piston, I found the syringe half full of a bloody serous fluid. Remembering the practice of Brickell, of New Orleans, who cured acute cases of intrapelvic effusion by the removal with the aspirator of clear serum, it occurred to me that the pressure of encapsulated serum in the centre of the exudation tumor might be the cause of the persistence of that tumor. I decided to remove it. I therefore inserted a medium-sized aspirated needle, and withdrew about two ounces of reddish, clear, inodorous serum. No reaction whatever followed this operation; the patient was left in bed for several days, and then allowed to go about as usual. Within a month the mass had shrunk to one-half its former size, and I then lost sight of the patient until nearly a year later, when she came to me for pain in the other side of the abdomen. An examination revealed nothing but a firm, fibrous band at the site of the former enormous swelling in the left broad ligament.

Case 2.—During the winter of 1878–9 a woman came to me for menorrhagia. I found a large sub-involuted uterus, with gaping cervix; suspected vegetations, and proved their presence by diagnostic curetting. I removed them thoroughly at her home, and painted the cavity with tr. iodine. In this I was assisted by Dr. Fruitnight, who I had since ascertained was the family physician. The hemorrhage, however, continued, and I therefore made a very thorough application of fuming nitric acid to the endometrium. This cured the menorrhagia, but induced a pretty sharp attack of cellulitis in the left broad ligament, which completely filled the left half of the pelvis with an exudation. This deposit resisted all remedies, and the patient was confined to her bed for nearly three months. Finally, the obstinate persistence of the tumor, the smooth surface and the cachexia of the patient led me to suspect deep-seated pus. I introduced the large aspirator needle, but did not hit the right spot at once. I then introduced the needle slightly to one side, and was rewarded by a few ounces of purulent serum. This was followed a few days later by a copious spontaneous discharge of pus, and from that moment the deposit began to melt away, until it almost entirely disappeared. Recently she had a fresh attack, which did well without active treatment.

Case 3.—In June, 1879, I was requested by Dr. Isaac Oppenheimer to see a patient of his who had intentionally produced miscarriage in the second month, some three months previously. She was promptly attacked with cellulitis of the left broad ligament, which confined her to her bed until I saw her. Dr. O. had only recently taken charge of the case, and found her in very poor condition—pulse 110–120, temperature 102° in the vagina, cachectic, icteric color of skin, frequent, almost daily rigors of but moderate intensity, great debility, and all symptoms indicating gradual septic infection, which Dr. O. thought must depend on pus retained in the exudation. Examination revealed a hard, brawny mass of the size of a fist in the left half of the pelvis, which was almost painless, and in which absolutely no fluctuation could be felt. In spite of the dense feel of the mass, it was decided to aspirate in view of the general evidences of septic infection from some source. As it was found that the probably thick pus would not flow through a fine hypodermic needle, a medium-sized aspirator needle was at once introduced, being guided into the vagina and against the most prominent portion of the mass to the left of the cervix by the finger. As soon as the point was securely pressed against the spot selected, the needle was steadily pushed upward toward the centre of the tumor, which was crowded down and steadied with the other hand on the abdomen; the needle was first forced with some difficulty through a dense creaking mass for nearly an inch, as it seemed, when it suddenly entered a cavity in which its point could be freely moved about. The aspirator being then applied, nearly two ounces of thick, creamy, fetid pus were withdrawn. This patient experienced no unpleasant effects whatever; the chills ceased from that very day, the pulse and temperature went down, and she made a very rapid, uninterrupted, and complete recovery.

Case 4.—The fourth case came under my care last December, for pain in the left hypogastric region and left leg, for which she had been suffering since an attack of "inflammation of the bowels" two years previously. She was also very anæmic. I found precisely the same condition as in Case 1, a large brawny exudation mass filling the left side of the pelvic cavity and latero-anteriorly displacing the uterus. It was painless, but at several spots close to the cervix I could indent the mass slightly with my finger. Remembering my previous two experiences, I decided to aspirate the mass for serum or pus. I accordingly did so at the house of the patient, removing first a few drops of serum with the hypodermic

needle and then over an ounce of a thick, grumous, brownish substance with the aspirator, which substance was pronounced by Dr. Heitzman to be fattily degenerated pus. The patient was put to bed and told to keep at rest for several days, with hot poultices on her abdomen, as she was a very nervous and sensitive lady. She, however, felt so well that she arose the next morning and took quite a long walk, the result being a fresh cellulitis on the same side, which kept her to her bed for several weeks. This apparent misfortune, however, finally proved a blessing, for as the fresh exudation disappeared it took with it almost the entire old mass, and the patient has now entirely recovered. Indeed, I have not seen her for over six months.

CASE 5.—At the time I aspirated this last case I had a patient under treatment for a large fresh cellulitic exudation in the right broad ligament, who had complained of pain in the left side for several years before I saw her. She was treated for three or four months by the usual means, without in the least changing the size or feel of the exudation. Visiting her one day, after an absence of a week or more, during which she had used no treatment at all, I thought my finger detected a more than usually boggy sensation all over the mass, which was not at all tender. It should be stated that the patient's appetite was poor. There was no fluctuation whatever; merely a doughy, boggy feel on firm pressure through the roof of the vagina. I expressed the opinion that there was deep-seated pus there, and said I would remove it. But other business prevented me, and in the meanwhile Dr. Hanks was brought in by her friends to see her (she was a charity case), and asked if he thought there was pus in the pelvis. He examined and said he could find no trace of any, as he since has corroborated to me. When I came, after a few days, with my students to aspirate, I heard of this, but I was so positive that there was pus there that I unhesitatingly plunged my exploring hypodermic into the mass, and to my satisfaction withdrew a syringeful of creamy pus. I had in this case aspirated through the Sims' speculum, after assuring myself with my finger of the most doughy and impressible spot of the mass, and inserting the needle directly into that spot under the guidance of the finger. I now proceeded to remove the pus with the larger aspirator needle, and partially succeeded; but it became clogged by a coagulum and I was obliged to remove it. I then plunged in a long slender needle attached to my hypodermic syringe, but failed to withdraw pus. Surprised, I removed it and again introduced it slightly to one side, and re-

moved a syringeful of pus, but only one. Reintroducing the needle in another spot, however, I again found pus, and so on until I had inserted the needle eight times and removed seven hypodermic syringefuls of pus. I then concluded to desist, fearing reaction, and hoping, as the last syringe was not quite filled, that the supply might be exhausted. It was evident that in this case there were a number of small parenchymatous, miliary abscesses, which accounted for my exhausting the pus so rapidly in one spot and finding it again close by. In all nearly 2 oz. were removed. No reaction followed, but as the exudation had not materially diminished in two weeks, I reintroduced the small hypodermic needle three or four times without success. From that time on, however, the exudation rapidly diappeared, and within a month the whole mass, which had not varied in size for four months, had disappeared down to a mere trace, which I still found there a few days ago. The patient has grown stout and is perfectly well.

Case 6.—During the past spring, Dr. Oppenheimer again asked me to see a lady with him, for pelvic cellulitis after abortion several weeks previously. I found a lady in high febrile excitement, with a tolerably large pelvic cellulitis at the left side, which protruded the vaginal roof downward almost below the external os. The patient was so much excited that I declined to interfere on that day, although I felt sure that there was pus there. Three days later, the doctor asked me to see the patient again. I did so, and found the patient free from fever, and the local condition unchanged; there was no fluctuation whatever, even where I had certainly expected to find it,—at the prominent spot near the cervix. Into this projection I introduced the ordinary hypodermic needle several times without striking serum or pus. This was probably due to the shortness of the needle, or to my happening not to touch the abscess. Thinking that I had been mistaken and that there was probably no pus as yet, I decided to defer further operations until the doubtless speedy formation of pus. The usual hot poultices and hot vaginal injections were ordered. That very night, while evacuating her bowels, the patient felt something give way in her pelvis, and a quantity, estimated at several ounces, of yellow pus was found in the vessel. This purulent discharge continued for a day and then ceased. I have not seen the patient since; but about a month later I met her husband in the street, and he informed me that his wife had begun to improve from that day, and had entirely recovered. Although in this case the exploring needle failed to find pus, perhaps because the needle was

too small, its repeated punctures certainly must have opened the path for the discharge of the pus. Had the larger needle been used at once, no doubt the pus would have been found and withdrawn.

Case 7.—On July 20th last, I was asked by Dr. Tauszky to see a patient with him who had been suffering from a fresh attack of cellulitis, for three to four weeks. He thought that the anæmic cachectic condition of the patient, and the feel of the tumor indicated pus, and requested me to operate if I thought proper. I found a large convex tumor filling the right half of the pelvis, but could detect no fluctuation whatever, and so expressed myself. Still the swelling was boggy. The hypodermic needle introduced through Sims' speculum at once revealed yellow pus, and by the repeated introduction (four times) of the needle at different parts of the swelling, about four ounces of thick inoffensive pus were withdrawn by the large syringe. The abscesses were undoubtedly parenchymatous and scattered, as could be proved by the exhaustion of one cavity, and the reappearance of pus when the needle was reintroduced at a slight distance. The needle passed through fully an inch of dense tissue before reaching softer material. Dr. Tauszky informs me that immediate improvement followed the aspiration ; that the nausea and vomiting rapidly ceased ; the exudation diminished to one-half its former size, and the patient, who had been confined to her bed for several weeks, was soon able to walk about, and has since continued to gain steadily.

Case 8.—In a case sent to me in the early part of last July, by Dr. F. H. Bosworth, the aspirator needle, although introduced to the centre of the hard, dense mass in the left broad ligament, failed to strike pus ; only a small stream of clear blood followed. Indeed, at the first examination I had doubted the presence of pus, as the mass was too hard. Even in a patient so debilitated and cachectic as this one (she had lost 25 pounds since her illness), the repeated aspiration did no harm. On the contrary, Dr. Bosworth informed me about three weeks later that the mass had decidedly decreased in size, and the patient had left her bed and called on him. No doubt the large blister which I advised to be put on the abdomen after the operation, had a share in this improvement.

Dr. Bosworth informed me, only a few days since (November 1st), that he had examined the patient and found no trace whatever of the tumor. Patient was in perfect health and had regained her original weight. That this tumor, which had existed since her

confinement 15 months previously, should have disappeared entirely, surprised me, and I cannot help thinking that the repeated puncture of the mass, since which the improvement began, started up the nutritive change which finally resulted in its complete absorption.

Case 9.—In a ninth case which recently came to my office, a patient who, a year ago, had the most severe pelvic cellulitis I ever saw, ending in perfect absorption of the deposit without suppuration, I found on examination a firm, solid mass to the right of the uterus, evidently in the broad ligament. As the patient complained of pain in the back and right side, for which she came to see me, I suspected a return of her cellulitis. She gave no history of febrile action, it is true, but I have repeatedly seen large deposits of plastic lymph in the pelvic cavity, without the slightest febrile action or anything more than local pain (side and backache). The mass was a smooth, hard ovoid, without fluctuation ; but its smooth outline led me to suspect fluid in it. To determine the correctness of this supposition and the nature of that fluid, I introduced my long aspirator needle on the finger in the vagina, reached a cavity after piercing a moderate thickness of dense tissue, and affixing my large glass syringe, withdrew three and a-half ounces of clear serum. The tumor at once collapsed, and now after six months has not refilled. I believe this to be either a hydrosalpinx (although the wall was rather thick for a dilated tube), or either a cyst of the broad ligament, or an ovarian cyst ; probably the former, as it was very close to the uterus, and not as movable as a small ovarian cyst generally is. A microscopical and chemical examination left the diagnosis between these two latter cysts doubtful ; there were no ovarian corpuscles, but on the other hand, the fluid did not coagulate with acetic acid as broad ligament fluid should. No reaction followed the operation. I mention this case simply as an instance of aspiration of an intrapelvic, obscure mass. In this case, as in those of inflammatory serum and pus, the aspiration was curative as well as diagnostic.

The special points which I have desired to emphasize and describe in this paper, are: (1) the anatomical and clinical difference between pelvic peritonitis and pelvic cellulitis, and the very frequent impossibility of distinguishing between the two affections, for the very simple reason that they are often combined ; (2) the possibility of suspecting

the existence of serum or pus in an intrapelvic exudation by reason of the long unchanged continuance of that exudation, its rounded outline, uniformly boggy feel, or the presence of occasional softer indentable spots; further by the constitutional symptoms, such as anæmia, cachexia, perhaps rigors; (3) the safety with which such chronic lymph-exudations can be punctured and aspirated; and (4) the rapidity and certainty with which the removal of even *very small* quantities of clear serum or pus from such exudations by means of the aspirator, is followed by complete absorption of the mass, and a return of the patient to health and strength. The rapid, complete disappearance of the exudation in one case (Bosworth's), after puncture without removal of pus, seems also to show that the aspirator needle may stimulate the lymphatics and bring about absorption of a mass which had been stationary for months.

I wish to repeat most emphatically that I mean only the old, chronic, stationary deposits (the very chronicity of which calls for interference) and not fresh acute exudations, which I would aspirate and expect to benefit thereby. The puncture of a fresh exudation and the removal of a greater or lesser amount of clear serum, as recommended by Brickell, of New Orleans, I consider a decidedly risky proceeding, and one that I should almost invariably expect to see followed by a fresh exudation. But the removal of a mass of inspissated pus or clear serum, or pus from the centre of such an exudation, the thick walls of which the small mass of fluid has no chance of piercing, is but fulfilling a positive indication and removing the cause of persistence of the exudation and the cause of the cachexia.

It is this class of cases where the exudation tumor has persisted unchanged for months, in spite of all the ordinary remedies, and cachexia, debility, etc., show the possible presence of pus in the mass, which is also boggy and doughy to

the touch,—these cases then, and these only, in which a careful diagnostic aspiration, followed by complete removal of the pus, if found, is indicated, and for such cases only do I recommend this treatment.

To plunge the knife into one of these lymph deposits, through perhaps an inch of more or less dense tissue, as we should do in large and pointing pelvic abscesses, scarcely seems justifiable; nor is it necessary for the mere purpose of removing a few ounces of thick pus, which probably will not be reproduced.

I should scarcely be impelled to use the aspirator (special indications of early suppuration excepted) under two months after the beginning of the attack, since I have seen so many of these large deposits which seemed destined to suppuration, melt away or shrink and contract under steady hot applications and tonics, after two or three months. Hard, contracted, fibroid lymph deposits are, therefore, not fit subjects for aspiration.

At a meeting of the New York Obstetrical Society last winter, when this subject came up for discussion and I related my experience, Dr. Lusk said that he had aspirated many times, but had seen no benefit in a majority of cases, except when, accidentally, pus was encountered. That is precisely the point, but I claim that the finding of pus in such cases is not an accident, and that its presence in small quantity can be detected with a fair degree of certainty by the signs I have described; and that its removal is indicated, is obvious. Serum certainly does not act so decidedly as an irritant as pus, but in my first case its removal was followed by marked benefit, and it can readily be understood how a few ounces of clear serum in a cavity with distinct walls, can resist absorption and thus keep up the lymph deposits about it, which disappear when it is removed.

I believe this treatment more likely to come into play in

exudations into the broad ligaments than in intraperitoneal effusions, which latter, when they suppurate, either form large abscesses which are easily recognized and generally burst spontaneously, or produce a matting together of the intestines, with the formation of abscesses entirely hidden by the intestines, and therefore not to be diagnosed.

Still, if I had a dense intraperitoneal pelvic exudation, in which the boggy sensation led me to suspect deep-seated pus, I should not hesitate to plunge in the exploring needle, and if pus appeared, remove it by aspiration. Such an intraperitoneal effusion will be almost always directly behind the uterus.

The dangers of this aspiration I have shown to be very slight. Indeed, I have, in several other cases in the dispensary, plunged my small hypodermic needle into a pelvic exudation where I suspected pus and found myself mistaken, without the slightest reaction. A proper amount of caution and rest afterward, should, however, always be enjoined.

In choosing the spot through which to aspirate, the most dependent, most boggy part should be selected, where the finger can indent the tissue; the distinctly pulsating arterial branches in the fornix should, of course, be avoided and the needle should be thrust in at least an inch until either a cavity has been reached, or there is danger of piercing the whole mass. If the short hypodermic needle is used, no such fear need be entertained; but with the long needle, the mass might readily be pierced, and the needle enter the peritoneal cavity; I therefore prefer to guide the progress of the long needle with my other hand on the abdomen, steadying and crowding down the mass. The progress of the needle into the mass can thus be easily watched, and the sudden cessation of resistance when the point enters the abscess cavity is unmistakable.

If the operator has a Sims' speculum and an assistant at

his disposal, his best plan will probably be to expose the cervix and vaginal vault, and plunge the short hypodermic needle into the spot which his finger has told him to be the most likely to show pus. If pus or serum is found, he withdraws his needle and introduces the larger needle under guidance of the finger, with the patient on her back, and without speculum, and aspirates the fluid. To avoid this double introduction of a needle, and to simplify the operation, also, and chiefly because I feared that thick pus would not escape through the ordinary fine hypodermic needle and thus the diagnosis fail, I had a slender needle 10 centimetres (4 inches) long made, which has about double the calibre of the ordinary hypodermic needle. In this respect the needle is like any aspirator needle of corresponding size, but I had the attachment made to fit both my hypodermic syringe, and a larger glass syringe holding 120 grammes (4 oz.) I was thus enabled first to aspirate the mass with the long needle and small hypodermic syringe, and fluid having been found, remove the hypodermic, attach the large syringe, and withdraw the fluid. If its quantity exceeded the capacity of the syringe, I had merely to close the stopcock attached to the removable nozzle, detach the syringe, empty it, push forward the piston and reattach it, open the stopcock, and withdraw the rest of the fluid. Air was thus effectually prevented from entering. Or if the preliminary exploration seemed unnecessary, as might often be the case, I might pass the long needle, with closed stopcock, into the mass, then attach the large syringe, open the stopcock, and aspirate precisely as with the small syringe. The length of the needle and attachments permit its easy introduction without a speculum; it is well to guard the needle-point with a small cork until the vaginal roof is reached, when the cork is stripped off and the needle thrust into the pre-

viously chosen spot. The direction of the needle should be toward the largest diameter of the mass: in cellulitis of the broad ligament, upward, outward (from the cervix), and slightly backward. The point of insertion will be between cervix and lateral pelvic wall, rather closer to the cervix, but this spot will vary.

I had the large syringe made chiefly because I did not see the use of having a complicated aspirator apparatus and large bottle, when the contents of such deep-seated, obscure abscesses will scarcely ever exceed 60–100 grammes (2—3 oz.), and the syringe need therefore be applied but once.

In my first case I used a large aspirator needle, but its size led me to fear injury to a vessel or serious reaction, and I therefore had the smaller needle made which, I think, could hardly do much harm even if a vessel were punctured.

In this treatment of aspiration of obscure pelvic abscesses and indolent exudations, I am pleased to be supported by the testimony of two such competent gynecologists as Drs. Richard B. Maury, of Memphis, Tenn., and Angus Macdonald, of Edinburgh. Dr. Maury* not only advocates the free vaginal opening by trocar and knife of distinct abscesses as soon as fluctuation can be felt, believing very justly that in this manner the accidental rupture of the abscess into a dangerous locality is avoided, but he also uses the following language in reference to deep-seated abscesses:

"When we cannot by medical signs alone prove the presence of pus—as is often the case—but believe it to be present from the constitutional symptoms, chills, fever, night sweats, we should not hesitate to explore the pelvic roof, or rectal, or abdominal wall by aspiration, and re-

* *St. Louis Courier of Medicine*, Jan., 1880.

move the effusion without delay wherever found. * * * Should such an effusion into the pelvis remain unabsorbed three or four weeks after the beginning of the attack, and all acute symptoms have subsided, and especially if pain and a feverish condition be present, we should not hesitate to carefully aspirate the pelvic roof with a delicate trocar and remove the effusion. In practice we are often unable to tell from the patient's history how long the effusion has been present, especially if the case has been subacute or chronic from the beginning; but we may always with propriety aspirate if the condition is not one of acute inflammation, and we are satisfied of the inutility of remedies."

Dr. Macdonald* expresses himself as follows:

"A difficulty that was wont to exist, viz., uncertainty as to whether there is or is not pus in a given pelvic tumor, is now removed. We can without risk, if we employ ordinary care, aspirate any such tumor and determine with ease whether it contains blood, serum or pus. * * * The aspirator may be freely used in pelvic cases as a means of diagnosis, without risk of injury following its employment. * * * It is perfectly surprising with what freedom we may aspirate abdominal tumors without causing injury."

With this last statement I am inclined to differ somewhat, unless by "abdominal tumors" Dr. Macdonald means only pelvic extraperitoneal exudations. That we cannot aspirate, even with the hypodermic needle, intraperitoneal (ovarian) cysts with impunity has been sufficiently proved of late, and is now universally acknowledged.

Should the withdrawal of a certain amount of serum or pus from an indolent pelvic effusion not hasten its absorption, and the abscess fill again, the aspiration may be repeated, and if then still ineffectual, the cavity laid permanently open by puncturing with a long red-hot platinum

* *The Edinburgh Medical Journal*, June, 1880.

needle attached to Paquelin's thermo-cautery (a plan I have repeatedly seen Dr. Noeggerath practise successfully); or the abscess may be laid open by a free incision with the knife, a drainage tube introduced and frequent antiseptic injections made. Dr. Pallen, of New York, stated at a recent meeting of the New York Obstetrical Society that he had, years ago, treated such a case of abscess of the broad ligament by making a large incision through the vaginal roof, passing in the finger and breaking down the infiltrated cellular tissue; the patient recovered.

No doubt the permanent opening of such abscesses is the proper treatment if they persist in refilling after repeated aspiration. Still I should prefer the perfectly safe puncture with the Paquelin needle to the always rather hazardous free incision which may unexpectedly strike an artery, large branches of the circular being easily felt pulsating in the vaginal roof.

In some cases I do not doubt that the stimulus of the aspiration, even though no serum or pus is obtained, starts up the exudation to absorption with or without suppuration.

In order to accomplish the free evacuation of the pus and render the abscess-cavity easily accessible, Lawson Tait has recently, in six cases, opened "pelvic abscesses" by a large incision through the abdominal wall, turned out the blood clots and pus which they contained, washed out the cavity, inserted a large drainage tube, and had the satisfaction of seeing the abscess close and all the patients recover within thirty days. One important point about this treatment is that the walls of the abscess were stitched to the abdominal wall, in order to shut off the peritoneal cavity. In all the six cases the abscess seemed to result from extraperitoneal hematocele. The excellent results obtained by freely opening the deep-seated abscesses of perityphlitis through a large

abdominal incision certainly favor this rather bold practice of Tait's. But he himself, for the present, limits the operation to those cases in which a vaginal incision could not be so easily and effectually made. Obviously, vaginal aspiration, drainage and incision should be tried first in all cases of extraperitoneal pelvic abscess in which the pus does not positively point through the abdominal wall. Only when vaginal treatment fails, may the external method be adopted.

I do not claim any particular novelty for this method of treatment by aspiration, nor have I canvassed and referred to the whole literature on the subject. I am aware that Simpson, Barnes and many others have practised aspiration of pelvic abscesses, and that it is still a diagnostic measure with many gynecologists. I have merely desired to place it again before the public supported in a few successful cases, and to call attention to its value in the peculiar forms of chronic cellulitis here described.

In this paper I have touched upon but a small portion of the vast subject of intrapelvic effusions. There are many interesting and important points in the etiology, pathology and treatment which still require more thorough investigation, and none more so than the influence of the residues of pelvic peritonitis and cellulitis in the production of pelvic neuralgia, uterine displacement and fixation, ovarian encapsulation and consequent sterility and general anæmia. The treatment of these old pelvic indurations and adhesions is as yet one of the most unsatisfactory chapters of gynecological science.

THE FUNGUS OF SYPHILIS.

By I. BERMANN, M.D.,

BALTIMORE.

IN the winter 1878–9 I received from Prof. Zeissl a freshly-excised prepuce, containing an indurated (Hunterian) chancre. I had previously discussed with him the nature of the syphilitic contagium, and hoped by microscopical examination of this absolutely fresh specimen to obtain further knowledge on this highly important subject. The results were far above my expectation, but before speaking of them in detail I would call attention to a very valuable paper by Prof. Klebs of Prague, published in the *Archives of Experimental Pathology and Pharmacology*, vol. x, p. 161, entitled: "The Contagium of Syphilis; an Experimental Study."

An account is here given of the results of inoculation of a monkey with schizomycetæ (or micrococci and bacteria), obtained by cultivating the tissue of an excised *non-ulcerated* hard chancre.

The method of cultivation is the usual one: A small fragment of the above, together with some cultivating fluid (such as Pasteur's), is brought into a perfectly clean glass tube, drawn out into fine points at both ends, which are then hermetically sealed over a flame. This tube is then kept in an ordinary breeding oven for several weeks, whence it is removed from time to time in order to examine its contents.

At last, in successful cultivations, a small film appears on the surface of the fluid, which the microscope shows to consist mainly of bacteria and micrococci. With the fluid thus obtained a female monkey, among many other animals, was inoculated, and in the usual time (about six weeks) an outbreak of genuine syphilis followed, accompanied with all the attendant phenomena. (For further particulars see original.)

Before the publication of Prof. Klebs' paper I had already, on examining microscopically the fresh prepuce obtained through Prof. Zeissl's kindness, discovered certain fungoid growths in its tissue, for which I could not find any other explanation than that they were in some way related to the disease.*

I have also to thank Dr. Woodward for calling my attention to an extract of an article on "The Low Organisms Found in the Hard Chancre," by Pisarewski, in No. 32 of the *Centralblatt für Chirurgie*.

This paper of Pisarewski describes small organisms found by him in all the specimens of hard chancres examined. Yet he failed to find the more highly developed forms of these organisms which enable us the better to understand what is the nature of syphilis. His failure can be, perhaps, attributed to the method used, or to the fact that he examined the tissue only in the immediate vicinity of the sclerosis. The paper being published in the Russian language, I have had to content myself with the extract as above mentioned.

Although there were many appearances in these sections

* Since the above was written, Dr. Woodward of the Army Medical Museum in Washington was kind enough to show me some specimens of dysenteric ulcerations of the colon, which contained, partly in the tissue and partly in the lymphatic vessels, certain fungi described by him in the second volume of the "Medical History of the War," page 473. As these fungi are in a great many respects different from those I found, and are very similar to those found in the wounds of persons having died of septicæmia, I can only regard this as a further proof of the specificity of the fungus found in syphilitic affections.

which indicated that these fungoid growths are the real causes of syphilis, I hesitated to publish my discovery without convincing proofs obtained by extended and corroborating investigations. The material being difficult to obtain, some time elapsed before I was enabled to continue my researches. What first attracted my attention, apart from those well-known changes taking place in the initial sclerosis, was a singular collection of micrococci and fungoid growths, firmly adhering to, and partly filling up, the lumina of most of the lymphatic vessels. How firmly they were fixed can be seen in the fact that they withstood all the different manipulations necessary to enclose them in glycerine after having had the sections lie in turpentine. Some of the arteries contained these growths in different stages of development and intermixed with blood corpuscles. In every one of the hundreds of sections I have made, and which were stained and prepared by various methods, have I found these same low organisms.

Dr. H. Knapp told me, after I showed him my specimens, that he has frequently found similar appearances in retinitis hæmorrhagica, and that he believed them to be coagulated fibrin. As this eye affection is also frequently to be observed in amyloid degeneration of the kidneys, this would perhaps be a further proof of my views. I need hardly say, that the fungoid nature of these organisms (apart from Klebs' experiments) has been sufficiently established by me through all different microchemical reactions, so that a mistake for anything else is to be quite excluded.

The micrococci are principally to be seen in the lymphatics, where they generally envelop the valves. They are small, strongly refracting spherical bodies, and resemble those illustrated by Klebs. The bacteria, as this author describes them, I only find in a few instances, and *only* in the arteries. The size of these organisms corresponds with the figures

given by Klebs. The principal changes have taken place in the lymphatic system and chiefly at some distance from the initial sclerosis, and this explains why others have been unsuccessful in discovering these fungoid growths, they, probably, having confined their researches to the limits of the induration. Wherever the fungi are most prevalent in the lymphatics, there we also find that the greatest change has taken place in the appearance of their tissue. This, in some instances, consists in amyloid degeneration of the endothelium as well as of the surrounding tissue, so that it becomes very difficult to find any nuclei, which latter are brought out with great distinctness by the method I use.

The walls of the vessels are covered by micrococci, disposed in thick layers, while the valves of the lymphatics are also thickly studded with these, and, by their swollen and hardened condition, show that they have undergone an inflammatory process. It is to be observed that the nearer to the original lesion the more prevalent are the micrococci or sporangiæ, while at some distance from the induration we find the higher developed forms. They consist of a network of fine thread-like filaments, most of which are much longer than the bacteria cultivated and described by Klebs. Many of them show node-like processes diverging at almost right angles from the original stem. In some of the vessels these branched filaments are so thickly interwoven that they stop the circulation more or less completely, or must, at least, act as a filter for the fluid passing through them, retaining the more solid parts of it, and eventually they must produce a stoppage in the vessels thus affected.

I do not feel ready to join Prof. Klebs in classifying these organisms with the schizomycetæ, as in a number of the specimens they appear to have the structure characteristic of the myxomycetæ. The branched filaments, penetrating partly the walls of the vessels and forming a fine network in them, especially indicate this relationship.

This seems to me, besides, to be of secondary importance, my object being to give an explanation of the *materia morbi* itself rather than its species, and how the infection occurs. The latter has already been satisfactorily proved by Prof. Klebs. My theory of the disease is as follows:

The infection takes place by reason of a few germs or micrococci being retained in a lesion of the skin. They are taken up by the lymphatics, and here they increase and multiply,

Section of an arteriole containing the branching fungus and spores described in the text. Drawn from a preparation of Dr. Bermann, by Dr. R. W. Amidon. Objective $\frac{4}{10}$ Tolles.

spreading principally in these, and soon begin to obstruct the circulation in them. The consequence is an infiltration of the tissue surrounding these, and thus the induration is produced. In course of time they develop more and more; small particles of them get into the circulation of the blood, and are carried into different parts of the body. They take root at those points where the conditions are most favorable for their growth, and cause there, eventually, the

same changes as before described. Thus, they produce metastasis in various organs, but especially, in the beginning (that is, between 4–6 weeks after infection has taken place), in the capillaries of the skin, where they effect an extravasation of blood by stopping the circulation. We cannot find a better explanation for the brown spots which remain after the exanthem has disappeared.

This also appears to me to furnish a very good theory for the frequency of syphilitic iritis in a relatively early period of the disease.

The mode of circulation in the eye being different, in many respects, from that of other organs, germs contained in the blood or lymph might find better conditions for development, while a general invasion of the body by these organisms has not yet occurred.

In all cases where a majority of the vessels of the body are invaded, a change in the proper nourishment of the surrounding tissue must necessarily follow ; and we can readily understand that if the growth of these organisms, thus colonized, is not checked in time, their area of metastasis must greatly increase, and by an invasion of the fungi into the tissue surrounding the vessels a multiplication of symptoms is certain to follow. A strong confirmation of my theory seems to me to be that Dr. Otis, of New York, has come to the same conclusion, viz., that all the sequelæ of the initial sclerosis are to be explained by mechanical obstruction of the lymphatics, by a different route, that is, by clinical observations. The principal rôle in this drama is played, of course, by amyloid degeneration of the lymph and blood-vessels (caused, in the first instance, by the fungus above described), as I find it to occur already in the initial sclerosis six weeks after the infection has taken place. Thus, the practical results of my investigations would be as follows :

To prevent the development of the disease, with all its disastrous consequences, by means of cauterization, excision,—circumcision, if possible (as recommended by Auspitz and others),—is indicated. At the same time, since we have not any other therapeutic agent to destroy these fungi producing syphilis, mercury in some form must be resorted to at once. Since the lymphatic vessels are the principal seats of the fungoid growth, and since these are most readily reached by hypodermic injections, this mode of treatment seems to me to be the most efficient.*

I have tried to give the results of my investigations, thus far obtained, in as small a space as possible, and only drawn those inferences which to me appear to be most important. The only proof that appears to me to be still needed, the colonization of the fungus in all secondary syphilitic ulcers, I shall furnish in the next paper; the material having hitherto not been quite satisfactory, I hesitate before corroborative evidence is obtainable. Finally, I may be permitted to thank Dr. Clarke, of the Johns Hopkins University for the kind assistance given to me in revising the proof-sheets.

* When properly made, these injections are not painful, and never produce abscesses unless the syringes are unclean. Even in cases where a quick mercurialization had to be resorted to (for instance, in iritis specifica, where the gumma was touching the membrana Descemetii), I have had the most perfect results with them, never observing any relapses nor producing salivation, At the same time, we are thus enabled to derive the full benefit from any other kind of medicine (such as iron, etc.) given by the mouth, and do not run the risk of disturbing gastric digestion by giving mercury internally.

AUSCULTATORY PERCUSSION.

By T. A. McBRIDE, M.D.,

PHYSICIAN TO THE OUT-DOOR DEPARTMENT OF THE NEW YORK HOSPITAL.

IT is difficult to measure with any great degree of accuracy the extent of the outward surface presented by the several organs of the body by the methods commonly employed. I believe that it may be safely asserted that the practitioner does not attempt the measurement of the presenting surface of the heart, liver and spleen, except when very much increased or diminished in extent. In fact, measurements in centimetres or fractions of an inch are usually essayed only by the consulting expert or by the lecturer in the amphitheatre in the presence of students, and how often the measurements announced are at fault the post-mortem records abundantly testify. It is not to be denied, however, that some few are so expert as to be able to accurately outline the boundaries of many of the organs of the body, but this accomplishment is obtained only by long experience and constant practice and by the exercise of the greatest care and patience. The method which has been most commonly employed has been that of mediate or immediate percussion, but since errors in the measurement of the presenting surfaces have been so frequent other methods have been proposed.

Dr. Griffith (*Lancet*, vol. ii, 1871, p. 848; vol. i, 1872, pp. 42 and 494) has suggested that a thin bow of steel,

with the ends connected by a taut catgut, might be used for differentiating subjacent parts of differing density. The catgut is made to vibrate, and the arch of the bow is then placed successively on different parts of the surface of the body, and as it is placed over an organ or viscus of dense consistency or the reverse, a change in the intensity, pitch and quality of the vibrations may be observed. It is known as the diapason. Dr. Herman Baus ("Phonometrische Untersuchungen der Brust und des Unterleibes," *Deutsches Archiv für Klinische Medicin*, vol. xi, p. 9, 1872) has invented for this purpose the phonometer, which consists of a tuning-fork fastened to a narrow base of metal, so narrow that it can be placed in the intercostal spaces. The principle of its action is the same as the diapason. The dimensions obtained by the diapason and phonometer are invariably smaller than the actual measurements made post-mortem, and the outline cannot be defined so sharply as to prevent a great source of error in the hands of different observers. The resonator of Gerhardt (Lehrbuch der Auscultation und Percussion, mit besonderer Berücksichtigung der Inspection, Betastung, und Messung der Brust und des Unterleibes zu diagnostischen Zwecken, 3d edition, Tubingen, 1876; pp. 81, 116, 161 and 272) and the sensitive flames of Wintrich (Einleitung, p. 44) demand but a passing notice since they are applied in the same way and act on the same principle. It is not likely, therefore, that these methods will be adopted.

In 1840 Drs. Camman and Alonzo Clark, of this city, published some observations in the *New York Journal of Medicine and Surgery*, for July of that year, on a method for the determination of the extent of the outward presenting surface of certain organs and viscera of the body. This method was a combination of auscultation and percussion applied by the aid of a peculiar stethoscope; and to this

method they gave the name of "Auscultatory Percussion," and it has always seemed to me, in view of the extremely accurate results which Drs. Camman and Clark obtained by this method, that a sufficient prominence had not been given to it in the practice of the day; and the object of this paper is to call attention to this method, and to a modification of the stethoscope which will enable any one to apply it individually and unassisted and without difficulty, since the success and utility of every method employed in observation depends greatly upon its simplicity and readiness of application. Before entering upon the consideration of the particular method of Drs. Camman and Clark, it will be well to briefly refer to some methods which have been suggested for the diagnosis of certain pathological lesions accompanied by changes in the density of organs, and for the estimation of the extent of the presenting surface of organs, to which the name of auscultatory percussion or synonymous terms have been applied, since the same principle is involved in all of them.

Roussel (Revue de thérapeutique médico-chirurgicale, 1er juillet, 1876) and Guéneau de Mussy (*L'Union Médicale*, 3d serie, 1876), under the title of "Auscultation Plessimétrique," called attention to auscultatory percussion, practised by percussion over the dorsal spines while auscultation was made over the front of chest, and the reverse percussion being practised over the clavicles, sternum or thorax, while auscultation of the posterior surface of the chest was made. By this method slight degrees of consolidation and enlargement of the bronchial glands were appreciated, which otherwise might easily have been overlooked.

Zuelger (*Berlin. Klin. Wochenschrift*, 43, p. 636, October 22, 1877) also called attention to a method of determining the presenting surface of the heart in which percusssion

was made over the præcordia while an assistant auscultated the left posterior surface of the chest.

Lücke (*Archiv f. klinische chirurgie*, von Langenbeck, t. xxi, fasc. 4, p. 838, 1878) directed attention to auscultatory percussion as an aid in the diagnosis of certain affections of the bones, and of fractures where slight or no displacement has occurred.

Hueter (*Centralbl. f. die medicin. Wissenschaften*, Nos. 51 and 52, 1878) has contributed to the same subject, employing the microphone or stethoscope of Voltolini.

In the method of Drs. Camman and Clark the stethoscope was a solid cylinder of finely-fibred wood, shaped in the direction of its fibres, about six inches in length and some ten or twelve lines in diameter. At one end was the ear-piece; at the other the instrument had the shape of a truncated wedge, so that it could be applied in the intercostal spaces without touching the ribs. The instrument was applied in the following way: The ear end was accurately fitted to the ear, and the wedge-shaped end was applied firmly and evenly over that portion of the organ to be examined which was most superficial and was not covered by any of the adjoining organs or viscera. Percussion was then made from some distance from the stethoscope toward it, and a change in the intensity, pitch, and quality of the percussion note answered to the density of the subjacent organ or viscus. The authors claimed for this method:

1st. "That the heart can be measured in all but its antero-posterior diameters, under most, perhaps all circumstances of health, and disease, with hardly less exactness than we should be able to do if the organ was exposed before us."

2d. "That the outlines of the liver can be traced with much greater certainty than by ordinary percussion in cir-

cumstances of health; and that it can be circumscribed in many conditions of disease in which ordinary percussion is not applicable."

3d. "That the dimensions of the spleen can be ascertained in circumstances that baffle ordinary percussion."

4th. "That by it we can mark the superior, inferior and external limits of the kidneys. Ascites presents no obstacle to the measurement of these organs, and from enlarged spleen the left kidney is easily distinguished."

Dr. Clark, in his lectures at the College of Physicians and Surgeons, states that in these investigations the limits of presenting surface of the heart were marked out by long needles thrust through the anterior wall of the thorax, and that it was the rule to find that the needles had passed through the pericardial sac by the side of the heart, or had just penetrated the edge, so to speak, of the heart.

Such accurate measurements having been made, it would seem that the method would have been generally adopted and commonly employed. The explanation of its not having come into common use since the demonstration of its value is, that two persons were necessary in the practice of the method,—one to practise auscultation, and an assistant to make the percussion.

In 1871 I observed Dr. John T. Metcalfe use the ordinary binaural stethoscope of Camman to outline the limits of a large liver by auscultatory percussion. The pectoral extremity of the stethoscope was placed over that portion of the liver which extended below the free border of the ribs, and percussion was made by the free hand of the auscultator, and the upper and lower borders located. Using the binaural stethoscope in this way, an enlarged liver or spleen, and even the stomach or large intestines, can be very accurately marked out, so long as the pectoral extremity of the instrument is placed squarely over the part to be defined,

and is not in contact with a rib or any substance of greater density than the subjacent part.

Encouraged by the results obtained in defining the abdominal viscera, I had a small, narrow, triangular pectoral extremity, about six millimetres wide, made to fit Camman's binaural stethoscope (Fig. 1). I had hoped to be able to place this narrow pectoral end in the intercostal spaces and avoid the contact of the ribs, but except in persons very much emaciated, and even in them, if placed close to the sternum, I found that this small pectoral end touched the bony parts, and, in consequence, was not to be relied upon.

FIG. 1.

I then obtained a wooden stethoscope (Fig. 2), con-

FIG. 2.

structed by Mr. Ford, of Messrs. Caswell, Hazard & Co., and consisting of two pieces of cedar twigs, with the bark on, which were steamed and bent to the shape of a binaural stethoscope, and with this my two first observations were made. The objection to this instrument was its great clumsiness, though I believe that wood is the best material from which to construct a solid stethoscope. The instrument with which the remaining observations were made is represented in Fig. 3. It consists of two solid pieces of hard rubber connected by a rubber hinge (metal was

first used as a hinge, but it embarrassed the conduction). The pectoral extremities of this solid stethoscope are three millimetres square, and can be placed in the intercostal spaces without touching the tips, and can be brought sufficiently near the sternum without being brought in contact with the cartilages or bones. In proving this method with the modification in the stethoscope, the *modus operandi* was as follows, and I may here mention that the application was made only to the mensuration of the anterior surfaces of the heart and liver.

[In an addendum in a future number of the ARCHIVES, results concerning the method applied to other organs will be given.]

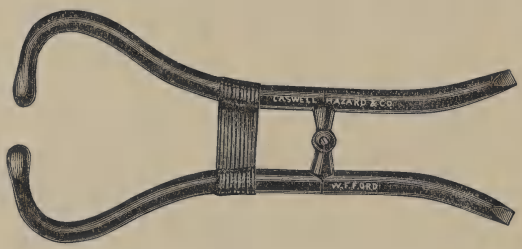


FIG. 3.

In marking out the anterior surface of the heart, the pectoral extremities of the binaural wooden stethoscope were placed over the fourth and fifth intercostal spaces close to the left of the sternum, and percussion was made from the periphery to the præcordia by the auscultator, with closed eyes, by means of the percussion hammer, or index and middle finger of the right hand, indifferently. As soon as a change in the pitch and intensity of the percussion note was observed, the pitch becoming higher and the intensity increased, the spot where this change occurred was marked, and a sharpened knitting-needle, ten inches long, was thrust in at that point. This

was done in each intercostal space of the præcordia to the right and left of the sternum, and in the right mammary line at points which were supposed to correspond to the upper and lower borders of the liver, and also in the mid-sternal line at the point supposed to indicate the lower border of the liver.

1st Observation. *Heart.*—In the second left intercostal space, the needle passed through the left ventricle, at a quarter of an inch from its free border. In the third left intercostal space, the needle passed through the left ventricle at about a quarter of an inch from the border.

In the fourth and fifth left intercostal spaces, the needle passed between the layers of the pericardium. In the fifth, just opposite the apex of the heart.

To the right of the sternum, the needles in the third and fourth intercostal spaces passed between the layers of the pericardium.

Liver.—In the right axillary line, fifth intercostal space and in the right mammary line, fifth intercostal space, the upper border of the liver was accurately defined by the needles; they just passed above it. In the mid-sternal line, and right mammary line, eighth intercostal space, the needles pierced the substance of the liver, and were an inch or more too high.

2d Observation. In this case the heart was displaced very much to the right, by air and blood in the left pleura. Many of the ribs were fractured, and some 800 c. c. of blood were present in the left pleura. I have, therefore, not included measurements of heart in this case. Needles, however, were introduced at the upper border of the liver in the right mammary line, and at the lower border in the right mammary and mid-sternal line, and found to mark out the borders with accuracy, being exactly on its edge, yet not penetrating it.

The following observations were made with the solid rubber stethoscope :

3d Observation. Needles were introduced through the third, fourth and fifth left intercostal spaces, and through the right third and fourth, also through the fifth in the right mammary line, and below the free border of the ribs in the same line, and in epigastric region in mid-sternal line. The needles about the præcordia passed just within the substance of the heart, less than a quarter of an inch from its free border, with the exception of the needle in the fifth intercostal space opposite the apex, which passed through the layers of the pericardium. The needle in right fifth intercostal space passed just through the upper border of the liver. The needles below the free border of the ribs were found to be an inch from the edge of the liver.

4th Observation. Needles were introduced through left third, fourth and fifth intercostals, and on the right side through the third and fourth. The needles on the right side and at the apex passed through the pericardium by the side of the heart; those in the third and fourth left spaces entered the substance of the heart close to its left border. Needles passed through the upper part of sixth intercostal space, pierced highest position of the convex surface of the liver, not one-fourth of an inch from the surface. The needles passed through right mammary and mid-sternal lines in right hypochondriac and epigastric regions were more than an inch below the free border of the liver.

It will be noticed that the heart in three of the observations was very accurately outlined, except in observation second, and that was not a proper case on which to test the method; although, if I had been cognizant of the history, proper precautions in the examination would, I think, have given different results. The upper border of the liver was also accurately defined. Except in the second observation, the lower border was not marked out, but the needles were

an inch or more too low or too high. I believe the explanation of not having obtained accurate results in defining the lower border of the liver is to be found in this, that as the cadaver is placed upon the table on its back, the liver falls back from the anterior surface, or its lower border at least does not closely meet the ribs or abdominal wall, the intestines covering the free border. In the second observation, the ribs and abdominal wall were pressed against the liver, and results were very accurate. If the cadaver had been placed in an erect or sitting position, the liver would have been more closely in contact with the abdominal wall, and the lower border closely marked out. In the living, either in erect or sitting positions, correct results would be obtained.

I believe that by this method the busy general practitioner will be able, by the exercise of some caution and patience, to mark out the heart and liver, and most probably the spleen and kidneys, with a very great degree of accuracy. I have been much surprised at the omission of any notice of this method in many of the hand-books of physical diagnosis, and the slight mention made of it by others. Weil, in his otherwise excellent manual of percussion ("Handbuch und Atlas der Topograpischen Percussion," von Dr. Adolf Weil, Leipzig, 1877), only very briefly alludes to it. In this country, Dr. Loomis' work on "Physical Diagnosis" is the only one which gives any description of the method and its application. I believe that the method has fallen into disuse because, as formerly practised, two persons were necessary to apply it, and it has been with the hope of calling the attention once more to its great value, and to promote the daily use of the method, that I have devised the instrument which has been described in this paper.

I must thank Drs. Peabody and Delavan, the Pathologist and Assistant Pathologist of the New York Hospital, for opportunities in proving this method by the cadaver.

EDITORIAL DEPARTMENT.

THE CULTIVATION OF SPECIALTIES IN MEDICINE.*

By E. C. SEGUIN, M.D.

Of the many practical questions which present themselves to the minds of students of medicine, and even more forcibly to the minds of young graduates, few, I take it, are more interesting than the one: "Shall I practise medicine in general, or become a specialist?"

The importance of this question seems to warrant my making it the text of remarks on this occasion when the Faculty of the College have delegated to me the pleasing duty of bidding you welcome.

The growth of specialism in medicine is quite modern, I might say recent, yet its germ is ancient. For example, in the celebrated medical school of Alexandria and among Arab or Saracen physicians in the first six hundred years of our era, we find mentioned as special practitioners, surgeons, lithotomists, oculists, and midwives. On the other hand, I doubt not but that more than one of the venerable pillars of our alma mater, the senior professors who are with us this evening, clearly recall the time when there were no specialists in the United States; a time when all practitioners of medicine, somewhat arbitrarily divided into the classes of physicians and surgeons, knew all there was to be known of medical science, and successfully enough practised in a

* Address introductory to the session of 1880–81, at the College of Physicians and Surgeons, New York; delivered October 1, 1880.

corresponding general way. Then no one devoted all his energies to the critical study of changes in the human cuticle, or spent hours peering into eyes with a little mirror, and racking his brains over complicated mathematical formulæ to correct nature's failure to produce a perfect eye. No one made it his exclusive business to light up, expose, and more or less barbarously medicate the various cavities and recesses of the human body, and no one (worst of all, I have heard it said) gave up all practice except that in connection with the nervous system. Were those the better days?

In the last thirty years all this has changed. Quite an army of specialists has sprung up all over the world; one specialty after another has made formal demands for recognition in the midst of the profession, and in the faculties of medical schools. Indeed, the human body has been so parcelled out to suit the demands of study and practice by specialists and pseudo-specialists that there is probably no room to spare; and the general practitioner is seemingly justified in exclaiming: "Would these specialist neighbors of mine leave me nothing to do?"

I repeat that specialties and specialists have increased remarkably in the last few years, and, planting themselves in large cities, have demanded the exclusive control of such cases of disease as seemed to fall within the limits of their respective branches of practice, and at the same time claiming (often wrongly, I am sure) superior knowledge of such matters.

This rapid growth, the rather loud claims, and the apparent great pecuniary success of specialists, have, naturally enough, roused in the ranks of the profession at large some adverse criticism and opposition. It has become rather fashionable, I suspect, to conveniently ignore the successful diagnosis and practice of specialists, and to pick out and hold up in full view their mistakes and failures. Yet, gentlemen, I appear before you to-night, prepared to maintain that the growth of specialties has been, and is, of the greatest utility to medical science and to the welfare of the public; and, also, that the practice of a specialty is, under certain conditions, perfectly right.

The growth of specialties is justifiable on the ground of its having been a natural and an almost inevitable development.

No ambitious or ingenious physician planned the creation of a special practice, but specialties have slowly risen up in accordance with the demands of the age; an age of unparalleled accumulation of human knowledge and of wonderful fertility in means for the application of such knowledge to practical uses. In this general proposition are included a number of immediate causes of the growth of specialties, and some of these I purpose briefly to review.

1. Early in this century, a considerable number of physicians in Europe, seem to have realized that a life-time of study would barely be sufficient to enable them to become conversant with the enlarging mass of medical knowledge, and that such an universal knowledge, if attained, would not be thorough enough to fit them for universal practice. Besides, the time consumed and the mental energy employed in this general study, were incompatible with original research and progress.

Probably because of such ideas, together with the prompting of progressive genius, we find that certain members of our profession, without becoming special practitioners, began and carried out special studies, and in several instances these special studies have made their authors immortal.

For example, let me name Lænnec, in what we call physical diagnosis; Bright and Rayer, in diseases of the kidneys; Bayle and Esquirol, in so-called mental diseases; Abercrombie and Ollivier, in diseases of the brain and spinal cord; Hope and Bouillaud, in affections of the heart; Cruveilhier, in pathological anatomy; John Hunter, Bichat, Magendie and Müller in anatomy and physiology.

Each of these great men for years devoted almost all his energy to the cultivation of what then seemed the outlying fields and dark by-ways of the domain of medical science. Had their ambition been, on the contrary, to be walking encyclopedias of medical knowledge, what would we say of them to-day?

2. It is very probable that the methods of thought and man-

ner of work of medical men in the first third of this century were considerably influenced by the development of specialties in general science.

In previous times a few great men in each century had appeared with a master-knowledge of the whole of the science of their day. Such, for example, were Bacon, Linnæus, Buffon, and, to a certain extent, Swedenborg. The birth of the natural sciences in the troubled times of the latter part of the eighteenth century may be looked upon as a sort of revolt against this assumption of universal wisdom by a few, and the beginning of independent, divergent, special work by the many.

If we take up this movement in the first half of our century we see, as examples, chemists busy for years at different branches of their science; some searching by analysis for elementary bodies, or for alkaloids in plants, others attempting the synthesis of substances, others yet endeavoring to discover chemical products which can be immediately useful in the arts, etc. We note the development of zoology into a great tree of knowledge whose various branches,—comparative anatomy, ornithology, ichthyology, entomology, paleontology and anthropology,—engage the attention, the special attention of innumerable observers. Histology, animal and vegetable, has arisen as a separate science; and so has embryology. In other departments we see men devoting themselves for years or for a lifetime to the study of light, of electricity, of nebulæ and stars, of climate and weather, etc.

To close this enumeration, let us say that the great scientific progress of the last fifty years has been the result, in greater part, of specialized research. And in the same period the men who, having a vast store of knowledge, have attempted to generalize the labors of specialists are exceedingly few. Perhaps I do not exaggerate when I say that Charles Darwin is the only one whose efforts in this direction have been deemed deserving of universal acknowledgment.

How could medical men, medical scientists, in constant intercourse with the promoters of general science, escape the tendency to specialize their studies? How could medicine as a part of

science remain conservative and sluggish in those times of minute observation and analysis, of subdivision of intellectual work, and of hungry original investigation?

3. The unexpected assistance afforded to medical research and practice by the progress of physics and the mechanical arts. No more striking example of an influence of this sort can be adduced then the effect of the introduction of the ophthalmoscope by Helmholtz in 1851. This instrument was not an accidental finding, but a truly scientific discovery resulting from the application of mathematics and physics to the study of the human eye. From this period dates the formal appearance of the first specialty, viz.: ophthalmology, a specialty which has attracted to its study many of the brightest minds of our profession, which has accumulated discoveries upon discoveries, and, partly owing to its being largely founded upon exact sciences, has carried diagnosis to a remarkable degree of accuracy, and brought its various therapeutic measures to a rare degree of perfection.

The study of diseases of the cavities of the body, such as the nose, pharynx, larynx, and the more deeply-placed organs has been greatly advanced by the invention of examining and illuminating apparatus.

The microscope has no doubt facilitated the growth of many a fine-spun and baseless theory, but it has certainly done much to enlarge the domain of science in the direction of physiology, diseases of the skin and kidneys, tumors, etc. At the present time, by its means important researches into the relation between microscopic germs and diseases are being carried on by numerous competent observers.

There are still other reasons, not perhaps scientific, why physicians have been led to limit their practice to certain branches. One is the great amount of time needed to carry out certain procedures of diagnosis and treatment, as for example in ophthalmic practice, in electro-therapeutics, hydrotherapeutics, etc. Again, in the last twenty-five years there has been a marked tendency to attempt the amelioration of chronic and so-called incurable diseases. These praiseworthy efforts need much thought and time,

and can hardly be carried out by the busy general practitioner. Lastly, there is a strong popular demand for the services of specialists. Our patrons understand the advantages of concentrated study and large experience in limited fields of practice. The public seek special advice in the shape of consultations, or even place themselves in the hands of specialists for a time, without any disloyal intention toward their family physician, who is often a personal friend.

Specialties, in study and in practice, have been, I believe, of advantage to medical science.

By limiting their attention to specified branches of medicine, a considerable number of physicians have relieved themselves of the fatiguing cares and complex duties of general practice, and have thus obtained an amount of leisure time for study, and a tranquility of mind favorable to original research.

In this way they have been able to make a critical examination of the writings of other observers in their own and in foreign lands, to make and record minute observations upon the living human being and upon the dead body, to undertake physiological experiments and anatomical researches intended to afford a logical basis for pathological hypotheses, and for an attempt at more rational therapy, and, finally, to accumulate experience in the comparatively rare diseases which general practitioners can only see at long intervals of time.

The results of these special studies, in a variety of departments, are beginning to take shape before us, as an unfinished yet a promising monument.

Each specialty can now point with pride to the numerous discoveries made by its followers ; each can show a record of enthusiastic work, of keen discussions, of undoubted progress carried on or made public in its special organization or society.

The literature of each specialty has grown to be immense ; embracing systematic works, pamphlets, and periodicals in many languages ; and taxing to the utmost the industry of the specialist who means to be well-read in his branch of medicine. In this connection, I might incidentally remark that a knowledge of the

three great living languages is now almost a *sine qua non* of success in special study.

The various specialties have, few will deny, proved useful to the public. I believe that multitudes of suffering human beings have been relieved or cured by specialists in the last thirty years, and that many, if not a majority of these cases would not have been successfully treated by general practitioners, however learned and able they might have been. This proposition could be brilliantly supported from the records of ophthalmology, but every specialty can claim corresponding achievements.

By limiting his range of practice a physician in the course of a few years accumulates a large experience in the diagnosis, prognosis, and therapy of certain diseases, many of which are looked upon as quasi-incurable, and are almost shunned by the general practitioner.

Specialties are further useful to the public because they furnish peculiarly well-qualified consulting physicians and surgeons. The willingness of general practitioners to seek special advice is becoming more and more evident. Even with our awkward rules of consultation, there need not be, I believe, any hostility or friction between the family physician and the specialist. The few unpleasant consultations of which I have been cognizant had been made so by personal faults in the physicians concerned.

I would venture to suggest that, on the one hand, the specialist who is saturated with the belief that he is the embodiment of science in his department, and who believes that the general practitioner cannot and does not know much in the same field, and, on the other hand, the general practitioner who is constitutionally unwilling or unable to have his diagnosis corrected or reversed, or to yield to the greater experience of the consulting physician,— that both these men are equally ill-prepared for the delicate and important duties of consultation.

To these favorable comments I am compelled to add a few words of warning respecting the intellectual dangers which I believe attend special practice.

The first, or more evident risk, is that the specialist shall be-

come a routine practitioner. This is, however, to a certain extent inevitable and justifiable. If, for example, a dermatologist find that a certain ointment is perfectly successful in the treatment of some diseases, who shall blame him if he continue to prescribe the same ointment in similar conditions of the skin ? A given form of instrument is found best adapted to relieve a certain deformity or displacement, shall we apply the term routine practitioner, in any opprobrious sense, to the orthopedist who applies this instrument one hundred or more times a year ?

By no means. This is a necessary routine, a useful routine, and one which it would be unwise to break through for the sake of sham originality.

But when such routine practice lulls a man asleep to the progress of his art, when it makes him blind and deaf to the improvements of others, when it prevents him from experimenting and trying to find something better, something which shall cure more quickly or with less pain or annoyance, then routine becomes a vice. Perhaps the mental state of the specialist who thus rests upon his oars, good oars though they be, might aptly be called one of partial dementia—a condition in which the past is remembered and over-estimated, the living present is ignored, and the pregnant future unthought of.

A second danger in special practice is the tendency to acquire a belief in the specific potency of drugs, as contradistinguished from their use in accordance with indications furnished by the patient's actual condition. As examples, I may quote the indiscriminate use of quinia in periodical symptoms, or the now fashionable prescription of a bromide for insomnia, or the application of electricity for paralysis. Do we always pause to consider that some remarkably periodical symptoms are not malarial, but of nervous origin ; that insomnia is merely a symptom, which may depend upon various pathological conditions, and which is sometimes more quickly relieved by stimulants than by sedatives ; or that many cases of paralysis get well spontaneously, or advance fatally, regardless of our electrical apparatus ?

Closely attached to specific medication, is the graver fault which

I may term symptom-worship. Naturally enough, the specialist's attention is taken up with the very striking symptom which has caused the patient to consult him ; such as convulsions, headache, eczema, failing sight, aphonia, etc. Some physicians, I fear, at once prescribe a favorite remedy or application in obedience to a half-avowed belief in specific medication. Others give more time to the case, analyze it somewhat, and prescribe intelligently. But how many have the courage to thoroughly investigate the problem, and then base their special practice in the case in hand upon the solid foundation of general medical knowledge ? To do this consumes time, may call for delicate manipulations, and the acquired data must be submitted to a peculiar compound of inductive and deductive reasoning, in order to form a clear conception or hypothesis of the symptoms presented by the patient, as explained by general physiological, pathological and ætiological laws.

Without such an inquiry, how can we hope to construct a rational treatment ?

Allow me to repeat that symptom-worship and specific prescribing must flourish in due proportion to the neglect of general pathology by specialists. This idea is the substance of one of the chief arguments against the usefulness of specialties. It is claimed that the specialist is necessarily one-sided ; that he carries on his researches and his practice in a mole-like way, *i. e.*, working in a furrow and ignoring its relations to the general system of medicine. I think that such a charge is unjust when applied in a general way ; and I believe that, as years go on, fewer and fewer specialists will render themselves open to this serious accusation.

Having discussed the origin, utility, and dangers of specialties in medicine, there remain some practical deductions or advice to be addressed to you personally.

You will recollect that at the opening of my address I said that one of the important questions which agitate the minds of students and young graduates in medicine is, whether to become specialists or not.

Now, this question, like one or two others equally personal which will occupy your thoughts, I earnestly beg you not to be in any haste to decide. Pray do not, as the phrase unfortunately is, "take up a specialty," for it seems to me that few things can be more unfortunate than that a young man, whether student or graduate, should label himself a specialist in his own mind or in the world's eye.

On the contrary, let your aim for several years be to cultivate your profession in a general way with all the industry and the time which a providential lack of private patients will leave you. This, I need hardly explain, is to be done by systematic reading and study, by hospital experience, and by the general practice of your art. For, even if you are ultimately to become specialists, let me assure you that you cannot be too well grounded in general diagnosis, in general therapeutics, and in anatomical and physiological knowledge.

During these years of preparatory study and work, not by any means unpleasant years to look back to, it may happen that you become greatly interested in some one branch of medicine, that circumstances lead you to see many diseases of a certain class; and that you experience a real desire, an ambitious desire, to cultivate this specialty. Thus, and then, if surrounding social conditions are favorable, if your medical friends consider that you are wise in your choice, a career as a specialist is open to you. This is what I would call a physician's natural growth into specialism.

In contradistinction to the above rational process of first securing a thorough post-graduate medical education, and then carefully following one's intellectual bias in the choice of a special study or practice, I would hold up to you as a warning the course of those who, soon after graduating, with or without residence in a hospital, say to themselves, let us be specialists—oculists, dermatologists, gynecologists, or what not. A certain fashion seems to determine which of the specialties is to be "taken up" by these hasty wooers. Some years ago ophthalmology was the proper thing, later still neurology was sought after, now I suspect

(one can't be quite sure of contemporary movements) that gynecology is popular. I believe that such a course is a great evil for those who adopt it, for the unfortunate patients who fall in the hands of these pseudo-specialists, and finally harmful for the scientific reputation of other men who properly cultivate the special fields of medicine.

I have heard it whispered, pretty loud too, that some young men proclaim themselves specialists or "take up a specialty" under the delusion that a special practice is easy and very remunerative. Now, I am not disposed to deny that some few specialists are in the end handsomely rewarded, but who, save these favored few, realize what patient waiting, and what long-continued labor are implied by this success? Then, how many would-be specialists toil and wait, yet never come to be recognized as such by their *confrères* and by the public? There must be a mingling of remorse with great disappointment after having thus spent years in an artificial attempt to be a specialist without reward.

Yet, I do not wish to be understood as maintaining that the honest and well-prepared student of a specialty *must* succeed. No, gentlemen, there is no Sunday-school good-boy doctrine in such real life questions. The artificial, ill-grounded, relatively ignorant special practitioner may make money and even attain a certain distinction, while his neighbor, who has carefully and conscientiously worked his way along so as to be looked upon with respect by his associates and even quoted as an authority, may fall short of success. This is because there enters into the problem of success in the practice of medicine a personal or social element of great importance, and which studious, original, and independent men are very apt to ignore. The successful physician is nearly always something more than learned; he is personally agreeable to his patients.

Finally, in considering whether you are to be specialists or not, I would have you bear in mind the normal organization of the great professional body which you join on graduating.

The immense majority of our brethren are settled in the country, in small villages and small cities, and they of necessity must be

general practitioners. All honor to these men, forty thousand of them I presume, who labor day and night, to the best of their ability and knowledge, for the relief of their neighbors' ailments. We can see them, with the help of our imagination, floundering through snow storms in the North, plodding along on horseback in the scorching sun-heat of the South, venturing into malarious regions, treating and even nursing contagious diseases, missing their meals—in fact, often shortening their lives to prescribe for the sick at rates of remuneration which we in New York consider ridiculously small. What matter if these men do not know all the fine points in medical science, if they have never heard of the depressor nerve, or do not know the name of the laryngeal muscles, or if they cannot establish the minute distinctions between various spinal paralyses? What if several times in their lives of usefulness, placed face to face with an unique or a complicated case, without the help of special counsel, they do too little or even do wrong? Will any one regret it more than they? and again, who, even in the elect circles of medical centres, does not also fail sometimes?

This great mass of the profession in the country and in cities, of which the majority of you must ultimately form a part, I greatly respect, all the more because I am the grandson of a physician who, for more than sixty years, was a useful and respected general practitioner in town and country.

Evidently, only a few physicians can, in obedience to the law of supply and demand, be specialists; and these few are found grouped according to certain geographical circumstances. No one would venture to attempt special practice in a village or in a large town. Even in cities of from fifty to one hundred thousand souls there is barely a living for one specialist in each department. Usually, special cases in such thinly-populated regions are treated by one whom I may call, with no intended disrespect, the quasi-specialist. He is a general practitioner who has devoted time and pains to acquiring special knowledge and skill in the treatment of certain diseases. Other physicians are glad to send special practice to such a medical man if he be well qualified and honest in his professional relations; yet there may not be enough of such

work to warrant his relinguishing general practice. Even in large cities there are many excellent physicians who might likewise be classed as quasi-specialists, yet I cannot but suspect that their special practice is dwindling as the public acquire more liking for strictly special advice and care.

There is, besides, a rather unclassified sort of physician in large cities, who is, to parody Molière, a specialist *malgrés lui ;* or if you prefer it, a specialist *nolens volens.* This highly respectable gentleman usually expresses contempt for specialists ; he looks upon them as narrow-minded, half-blind men working in a rut. He himself is widely read in medical lore, he may be a sort of walking encyclopedia, and he has practised in all ways. Yet, fortunately for the public, though perhaps unfortunately for his grand ideal, this physician is known to his colleagues by his systematic work on this, or his lectures on that, or his monographs on various topics, etc. He is *de facto* a specialist ; his *confrères* and people generally know that his opinion is particularly valuable in certain affections, and his consultation practice is colored accordingly. If he be a professor or a writer, his lectures and books reveal what really is in him in spite of a show of universal wisdom ; and the bulk of what he writes is commonplace alongside of that past which treats of the topics he has unconsciously specialized.

Lastly, in all large cities there are the pure or strict specialists, that is, physicians who decline all practice outside of their specialty. I greatly hope to see this small class somewhat enlarged, mainly for the reason that we would then have a larger number of well-qualified observers with leisure to work, and thus a marked impetus would be given to original medical research in this country.

In this address I have endeavored to show that the growth of specialties has been normal, and in accord with the general scientific movement of the age ; that specialties are useful to the public and to medical science ; and that the practice of a specialty is not unattended by intellectual dangers.

I have neither urged you to become specialists nor advised you

to shun and contemn specialties; but have tried to make plain and forcible my notions, that the decision to become a specialist should be reached deliberately, upon a careful estimate of the tendencies and capabilities of your minds; and that your special studies and practice should rest upon a broad and solid medical culture.

Very few of you can expect to become strict specialists, and the career of all the rest as quasi-specialists and general practitioners will be equally useful, equally desirable socially, and equally honorable.

NEW BOOKS AND INSTRUMENTS.

Antiseptic Surgery. By W. McCormac, M.A., F.R.C.S.E. and I., Surgeon to St. Thomas's Hospital, etc. London: Smith, Elder & Co., 1880. pp. 8–286.

This work had its origin in the important discussion which took place last December before the British Medical Association—a discussion introduced by Mr. McCormac's paper, which is here reproduced, and stimulated by Mr. Lister's statistics, some of which had been printed a few months previously, while the remainder were embodied in Mr. Lister's remarks, made during the debate. Its importance, therefore, is great, and, taken in connection with Mr. Savory's address before the same association in August, 1879, it furnishes an admirable means for estimating the advantages of the antiseptic method. It is divided into five parts, as follows: McCormac's address, given in full, occupies the first 46 pages; the debate, which was carried on by Bryant, Barwell, Wells, Holmes, Paget, Lister, Hutchinson, and others less well known, fills the next 53 pages; then 15 pages are given to the antiseptic theory, 46 to the materials used in the dressings, and 20 to "antiseptic practice." It is, therefore, a very complete statement of the advantages claimed for the method, the data by which the claim is supported, and the details to be observed by those who wish to employ it. It is a book which every surgeon, and especially every hospital surgeon, should possess, and if Mr. Savory's address and Cheyne's statistics of Mr. Lister's joint cases could be bound up with it, it would form a manual of practice whose value could hardly be overestimated.

Surgical Diagnosis of Tumors. By Professor Lücke. Translated by A. T. Cabot, M. D. A. Williams & Co., Boston, 1880. pp. 43.

Prof. Lücke, who has been long and favorably known as the

author of the article upon tumors in Pitha and Billroth's "Handbook of Surgery," seems to be peculiarly well fitted to mark out the general lines upon which the diagnosis of tumors is to be conducted from the surgical standpoint, for he combines a knowledge of their minute structure which is probably not inferior to that of any professional microscopist, with a large clinical experience as a hospital surgeon. He excludes cysts from the class of tumors, defines the latter as an "increase in size through the new growth of tissue by which no physiological end will be gained," and classifies them in three groups according as they are composed, mainly of (1) connective tissue, (2) epithelial tissue, or (3) more highly developed tissue. Believing that tumors do not present transition forms from one type of tissue to another, that "tumors of the connective-tissue series remain always within the type of that tissue, and that epithelial tumors can only originate from epithelial cells," he makes the origin of a tumor a capital point in the diagnosis, and says the first and main question in every case is "in what tissue did the tumor originate?"

The second diagnostic point is found in a consideration of the region of the body within which the tumor is developed, and is illustrated by reference to the well-known frequency of carcinoma in the female breast, myxomas and osteo-sarcomas in the thigh, dermoid cysts in the testicles, ovaries, and edge of the orbit, etc. A third point is the recurrence of different kinds of tumors at different ages, and a fourth is found in the rate of growth.

The second half of the pamphlet is devoted to a discussion of the relations of a tumor to the adjoining parts, and their bearing upon the question of malignancy and the explanation of various symptoms, and to the information to be obtained by palpation. While the pamphlet contains little that is new, it presents the facts in a very compact and striking form—one that is convenient for reference—and well deserves to be read and pondered by every surgeon. [L. A. S.]

Photographic Illustrations of Skin Diseases. By GEORGE HENRY FOX, A.M., M.D., Clinical Professor of Dermatology, Starling Medical College, etc. New York: E. B. Treat.

To those who are removed from great centres of population, to whom sufficient opportunities for observing the manifold forms of disease to be encountered in the hospitals and dispensaries of large cities, are not afforded; to those who are engaged in active medical practice, and whose paths do not lead them where these

advantages may be availed of; to those whose calling it is to teach the art and science of medicine, the necessity of various forms of pictorial illustration, as adjuvant to the other methods of acquiring and imparting information, is continually manifested. Such aids to knowledge are more or less essential, in some instances answering nearly all the requirements of the subject to which they are devoted, and in others serving merely to imperfectly represent that which, did time and opportunity favor, should only be studied in a practical manner. When one desires to study an anatomical plate, he has no expectation of finding a faithful likeness of the muscles, the bones, the viscera; if, for artistic beauty the resemblance is made striking, his æsthetic sense is pleased and stimulated, but he is not taught more thoroughly than he would have been by a less realistic plate, but one displaying with mathematical accuracy the distributions and relations of the parts as they are in nature. In such productions more than this is not to be expected.

How incomparably more arduous is the task where the object is to represent Nature exactly as she presents herself to the eye of the clinician; to preserve for the library the revelations of the hospital ward. The difference is as between the diagram and the work of art. This task he who would illustrate cutaneous pathology must undertake; and the attempt is all the more difficult from the circumstance, that while the clinician is enabled to avail himself of all the assistance afforded by the touch, the varying conditions of moisture or dryness, of heat or coldness, of infiltration or swelling, that the pencil cannot depict, he who derives his information from the atlas must be dependent upon such help as the eye alone can give. Nevertheless, the demand for such illustrations has been so great that very many efforts have been made to supply it, varying from the utterly unintelligible frontispieces of older treatises upon skin diseases to the elaborate and costly works of Alibert, Hebra, and others. Of late years great advances have been made in this art, and chromo-lithography has enabled authors to furnish us many marvels of skill and truthfulness. It has occurred, moreover, to a number of dermatologists, that in employing photography in illustrating these subjects, it might be possible to reproduce the exact semblance of disease. Consequently, photographic atlases of skin diseases have been produced in various countries, but with indifferent success, the difficulties, heretofore having been too formidable to admit of satisfactory results.

Dr. George Henry Fox in undertaking this task anew has recognized the imperfections of previous processes, and has adopted a modified method of photography known as the artotype, whereby "the picture not only possesses the sharpness of detail and brilliancy of ordinary photographs, but, unlike the latter, will not fade through age and exposure to light." By supplementing this process with the color and pencil of the artist, he has endeavored to represent skin diseases with photographic accuracy, and at the same time as life-like as possible. The work is now completed, and we are able to judge of its merits as a whole, and to determine to what extent the author has been able to fulfil his promises. The atlas is in twelve parts, forming a quarto volume and comprising sixty-one photographs included in forty-eight plates. The affections represented are comedo, acne, lepra, elephantiasis, keloid, rosacea, psoriasis (nummulata and annulata), ichthyosis, fibroma pendulum, varicella, zoster (pectoralis and inguinalis), eczema universale, leucoderma, chromophytosis (tinea versicolor), favus (capitis and corporis), eczema cruris, eczema infantile, eczema papulosum, eczema ichorosum, eczema pustulosum, eczema squamosum, eczema barbae, eczema manuum, eczema e venis varicosis, ulcus varicosum, lupus vulgaris, lupus erythematosus, epithelioma superficialis, epithelioma rodens, epithelioma, trichophytosis (tinea trichophytina) capitis and corporis, lichen planus (2), lichen ruber, kerion, molluscum (2), erythema multiforme, phtheiriasis capitis, phtheiriasis corporis, scabies (2), porrigo e pediculis, herpes facialis, hydroa bullosum, erythema circinatum, erythema exfoliativum, purpura simplex, cornua cutanea, alopecia areata, morphœa, scleroderma, and sarcoma pigmentosum (3).

These plates are so unequal that it is difficult to estimate the value of the work as a whole. Some of the illustrations are simply perfect, certainly better than any thing of the kind we have ever seen. It would be difficult to excel the plates of comedo, ichthyosis, acne simplex, keloid, leucoderma, herpes facialis. Those of comedo and ichthyosis, for example, appear more perfectly truthful the more closely they are examined, the magnifying glass even adding new beauty. We think the keloid shown is exactly similar to this affection as it is often seen in the negro. Other plates are very good, but we regret to say, still others are positively bad. The series of nine plates, comprising ten pictures of eczema are fair representations of different forms and stages of this affection, not so satisfactory as could be desired, however; and one plate, that of eczema papulosum, quite

fails to convey the desired impression. Inasmuch as the work is intended for general practitioners and students as well as specialists, it is very unfortunate that under the titles eczema infantile, eczema pustulosum, eczema ichorosum, conditions that are nearly identical should have been shown. Such different names necessarily create the impression that much more widely varying processes are referred to, and this not being apparent to the observer, an embarrassing uncertainty is created which tends greatly to discourage the study of these affections. Eczema barbæ is represented very truthfully, and the same is to be said of eczema erythematosum. The picture of eczema e venis varicosis lacks the dark coloration that is usually observed, and that the amount of crusting should lead one to expect. A similar objection holds regarding the photograph of varicose ulcer. The hyperæmia is altogether too slightly marked. Eczema erythematosum is very well shown, but eczema universale is very unsatisfactory. Dr. Fox has not succeeded better with psoriasis than have those who have preceded him. It is certain that the two plates will hardly help him who desires to study this affection ; but, it must be confessed, the subject is one of great difficulty. With favus, lichen planus, rosacea, fibroma pendulum, chromophytosis (tinea versicolor), he has been more fortunate, and these affections are excellently well shown. So of molluscum, the observer will derive well-defined conceptions from the plate. The two pictures of lupus vulgaris and lupus erythematosus are too much alike. The differences are clinically quite noticeable, though here they fail to appear. The sebaceous-gland element of lupus erythematosus is too feebly represented ; but the central cicatrix in the midst of the lupus vulgaris is shown most admirably, and adds greatly to the value of the plate. Kerion is practically almost too rare an affection to justify its reproduction, especially as it is not the typical variety that is represented. Elephantiasis is shown in a very remarkable plate displaying the configuration of the affection, but leaving much to be desired in defining the more minute pathological processes. Lepra (maculosa and tuberosa) is well illustrated, though the coloring of the ulceration about the fingers of L. tuberosa is very dauby. The picture of scabies is likewise very correct, though this, as well as quite a number of others, loses much by the greatly-reduced size of the plates. The morbid appearances due to the presence of pediculi receive careful attention, though it seems unnecessary that the author should have employed two plates to represent "porrigo,"

a term employed to designate a form of impetigo contagiosa (according to the author) often present in persons infested with lice.

Erythema multiforme, as shown in the atlas, has the hemorrhagic element much more pronounced than is usual, while the convexity of the eruption has not been made noticeable. Zoster is likewise represented in an unsatisfactory manner ; the erythematous ground of the affection being much too livid, while the appearance of the vesicles and bullæ hardly fills one's ideas of these lesions. Varicella is well shown, though, as is unavoidable, the distended, perfectly-limpid vesicles of this malady are absent. It is probably impossible to exactly reproduce these lesions. Lichen ruber is not well illustrated. The eye receives no well-defined impression of the specific appearances of the disease. This is undoubtedly due to the very greatly-reduced size of the plate, a fault that renders the correct representation of the lesions impossible. If, instead of the whole figure of the patient, the bust alone were photographed on a scale more nearly approaching life size, much better results would have been attained.

The clinical differences between alopecia areata and tinea tonsurans ("trichophytosis capitis") are usually readily appreciable, yet in the pictures of these two affections the circular patches afford no other method of deciding between them than the attached descriptive titles. Where the examination of lesions can only be conducted properly with the most careful scrutiny and with an almost microscopic eye, it cannot be expected that ordinary photography can accurately represent them. The distance at which the subject requires to be situated from the photographic apparatus destroys the sharpness of outline essential to the diagnostician. Ringworm of the body likewise fails to find satisfactory illustration ; one must read the appended title before he can identify the affection. Passing to the examination of the plates of epithelioma, that of the superficial variety, while lacking definiteness, gives a truly admirable idea of the lesion and of its usual attending conditions ; but the adjoining photograph of epithelioma rodens is spoiled by the triangular daub of color by which the artist rather obscures than assists the diagnosis. The epithelioma of the lower lip is an excellent and truthful likeness. Cutaneous horn is shown in a very remarkable photograph ; the horns are situated upon the lip, which has evidently become cutaneous. The coloring of the ulceration is bad. Morphœa and scleroderma are placed side by side, but the plate can convey no clearly-defined

impression of the processes to one who is not already acquainted with their characteristics.

Taken as a whole, the work is certainly a good one; but the author has sacrified much of its value in the attempt to accomplish too much. Take, for example, the two last illustrations. How can one hope to depict lesions, the diagnostic symptoms of which are to be recognized, not by the eye, but by the touch. Sclero-derma, we take it, is not and can never be a fit subject for pictorial representation. An atlas for the illustration of skin diseases, to be most useful, must be limited to those diseases that present their most characteristic features to the eye; all others had better be disregarded. An artist may paint the nose of Bardolph, or even put a toothache upon canvas, but he would hardly attempt to idealize a pneumonia.

If Dr. Fox had limited the scope of his undertaking the result might have been the very best production of the kind that has yet appeared. Some of his plates are inimitable. Upon examination one finds that the most perfect plates are those where, the essential pathological processes being appreciable to the eye, the coloring of the artist is least intense. Where, indeed, color must be laid on after the photographic apparatus has done its work, in the operation, the accuracy of the likeness must often be destroyed. Again, each illustration, requiring separate treatment, is much at the mercy of the imagination, since it is impossible that the artist can always work with his model before him. That this objection is one of great moment, can be readily demonstrated by a comparative examination of a series of plates of this atlas, when it becomes quite apparent that the execution is very unequal. The same illustrations show decided variations in different copies of the work. This is a very grave fault, but under the circumstances, it must be confessed, almost unavoidable; and when such inequality of execution exists, the task of a critic becomes most embarrassing, for he may condemn a plate in one copy of a work which in another may be unobjectionable. As photographic productions the plates show this variation to a minor degree; for instance, in two photographs representing the same illustration of fibroma pendulum, there is a difference in definition that makes one a good, the other a bad photograph. This objection holds true of other pictures. The variations in coloration are, however, much more noticeable. A few examples will show this. Chromophytosis (tinea versicolor) in one atlas is of a dark-brown color, certainly darker than we have ever seen,

while the similar illustration in another atlas has hit off the shade almost to perfection. The contrast between the two is remarkable. Likewise, one plate of favus shows admirably the pale, characteristic sulphur color, while in another this is hardly perceptible, the tint being more grayish as in another stage of favus. In two plates of eczema squamosum, there is also marked difference in the intensity of coloration. The ulceration in the illustration of cornua cutanea is of a dull, light-brownish shade in one specimen, in another it is of a dingy pink. Morphœa in one instance contrasts sharply by its dead-white color from the healthy skin, while in another the line of demarcation is only demonstrable by the pink border drawn by the artist. Eczema papulosum in one plate to some extent resembles this affection in the living subject; in another, one cannot help seeing the dots of paint. Purpura simplex has here a brownish-violet color, there it is distinctly lilac.

There are a few instances, however, of even more serious discrepancy than these, where the artist has brought out features in some pictures that are indistinguishable in others. The most important example of this is in the illustration of scabies, where, upon the dorsal surface of the middle finger, there are two delicate, dotted, sinuous lines, unmistakable representations of the furrows made by the female acarus, in one picture, while in another similar picture of another series of plates, it is only by the exercise of the imagination that these features become visible. Examining the latter plate alone, one would never be aware of these lines. A similar omission is to be complained of in the illustration "eczema e venis varicosis," where the presence of an ulcer in one photograph is clearly demonstrated, while in another it can only be detected with certainty by careful scrutiny; it is shadowy and indistinct.

Such, we think, are the most important imperfections of this work; they are undoubtedly serious, but nevertheless, not sufficiently so to destroy the great value of the whole, which is a most important contribution to dermatology; in some of its features, unrivalled. In selecting such a wide range of subjects, Dr. Fox has attempted too much; for the very nature of some of them precludes the possibility of their successful representation by photographic art.

Each plate is accompanied by two pages of letter-press, and great skill has been exercised in condensing into this space so much that is requisite for the proper understanding of the sub-

jects. The author has succeeded in including within these limits, a short and concise history of the case, a general account of the affection, with essentials for diagnosis and treatment, the whole necessarily brief, yet comprehensive and in accord with the most advanced teachings of the dermatology of the present day. The selection of subjects, as we have shown, has not been very judicious. The practitioner would derive greater advantage from other illustrations than those of fibroma pendulum, sarcoma pigmentosum, lichen ruber, kerion and other affections but rarely encountered ; though the importance of recognizing them when they do occur, may, perhaps, justify their presence in the atlas. Those not appropriate for photographic illustration have already been referred to. In representing various conditions due to phtheiriasis capitis and corporis, we think the author has acted wisely, as the appearances are quite characteristic. It is not at all clear, however, that he has done right in resurrecting the now neglected term "porrigo," one that has received so many interpretations that dermatology could only extricate itself from the confusion by abandoning it altogether. It is by no means certain that there is a contagious affection, such as Dr. Fox describes under this appellation, sufficiently distinct from impetigo contagiosa to be entitled to a separate name. Indeed, Fox considers the affection identical with this latter, and under these circumstances his reasons for further complicating dermal terminology are not obvious. But in the case of "porrigo" resulting from the irritation of lice, we suspect that the process is more often eczematous than otherwise.

Chromophytosis and trichophytosis are terms to which objection may likewise be made. Although of themselves very excellent and expressive names, they have as yet received no general sanction, and it is better that dermatological nomenclature should not be further amplified, unless advantages, greater than in these instances, can be offered. The reader is not informed in the plate or in the text that chromophytosis is the same affection as the tinea versicolor and pityriasis versicolor of older writers. It is only in the classification and momenclature that the synonyms are given. This classification is based upon that of Hebra, and is like those of Duhring and Bulkley, differing from these, however, in arrangement. (I. E. A.)

The Practitioner's Hand-book of Treatment; or, The Principles of Therapeutics. By J. MILNER FOTHERGILL. Sec-

ond American from the second English edition. Enlarged. Philadelphia, Henry C. Lea's Son & Co.

Dr. Fothergill opens his essay with one or two remarks that at once enlist our interest and our sympathies : "It is eminently desirable that a medical man be generally well-informed ; but what is to be still more devoutly wished for is that he shall be a skilful practitioner." "It is all very well for a patient to feel that his medical man is a gentleman ; that he is carefully trained in physical examination, and capable of constructing a skilful diagnosis ; who has added a residence abroad and the observation of foreign schools to his home-acquired attainments ; but the essential thing, after all, is confidence in his power to aid him when stricken and prostrated by disease or accident."

The writer then proceeds to enter a gentle, but, as it seems to us, rather superfluous protest against "the firmly established fear" existing "in the minds of some, both in the profession and out of it, that there is something dangerous and unsafe in too much understanding of the nature of things, including the nature of disease." Preoccupation about such an idea, suggests a curious nearness of sympathy between the member of a learned profession and the uninformed laity. This community of feeling may be the reason for the decidedly democratic absence of technicality in the book. This is so often noticeable in English medical books, from either side of the Atlantic, that we may ask whether it be not a result of free political institutions.

In his preface, Dr. Fothergill tells us that "this work is not an imperfect practice of physic, but an attempt of original character to explain the rationale of our therapeutic measures." The field for the operation of therapeutics is divided into the following sections : assimilation ; excretion, body-heat and fever ; inflammation ; anæmia, plethora, congestion ; growth and decay ; abnormal growths ; blood-poisons ; acute and chronic disease ; diabetes, rheumatism and gout ; diatheses and cachexiæ ; action and inaction. Then follows a series of chapters on diseases of different systems,—the circulatory, respiratory, digestive, urinary, reproductive, cutaneous, lymphatic, and nervous. The volume concludes with three supplementary chapters on public and private hygiene, on food, and on the general conduct of the physician toward his patient. This last winds up the treatise, precisely in the same way as manuals for students so often conclude with a chapter on cleanliness of the person, the use of the tooth-brush, etc., etc.

The first criticism to be made, both upon this division of subjects, and on the way in which they are treated, is that it is both redundant and incomplete. It has, indeed, precisely that mixed character of scrappiness and of redundancy which may legitimately belong to the genial after-dinner talk of a successful practitioner—a man whose senses are becoming a little dulled, and who therefore is inclined to pooh-pooh at the refinements of physical diagnosis ; a man who reads foreign authors in translations ; who is not particular about the relative dates of investigations ; whose estimate of values is a good deal an affair of proximity ; who in diagnosis lays much greater stress on "experience" and "medical tact" than on rigid analysis of the conditions of the problem.

In his preface, Dr. Fothergill remarks that the great difficulty in writing such a book as he proposes, is to know not only what to put in, but what to leave out. The recognition of such a difficulty ought surely to have led to the omission of all perfectly well-known details, all commonplaces, truisms and platitudes. It should have prescribed the selection of only such original views, such incisive suggestions, such incidents of personal experience, such happy applications of recent scientific discoveries, as it is the especial business of the sagacious clinician to make, and which permit of enunciation in a less systematic form than do the technical researches of scientists themselves. Now, had Dr. Fothergill's book been composed exclusively of such material, its usefulness would have been doubly increased ; for time would have been saved in reading quotations from familiar authors, and the volume itself would have been so reduced in compass that it might have been carried about in a vest pocket. Dr. Fothergill, however, was bound to write a hand-book of a certain size. The original material would not expand, and the numerous vacua between the book covers have been, necessarily, filled with stuffing. Thus the really useful paragraphs are almost buried out of sight. It is like a boarding-school pudding—too much dough for the raisins.

Let us pick out the latter first. Taking together the chapters on assimilation and on the digestive system, which overlap and even repeat each other too much to be separated, we find the remark on the power of ipecac. to increase the vascularity of the stomach, and on its inferred utility in atonic dyspepsia ; on the value of water in washing away waste products (a remark belonging to and repeated in the chapter on excretion) ; on the clinical fact that iron often fails to agree with old people, and on the other

fact that Hughlings-Jackson now admits Brown-Séquard's proscription of iron in epilepsy; on the value of opium in bulimic dyspepsia; on the functions of the liver in the metabolism of albuminoids, and on the importance of considering such functions in the treatment of lithiasis; on the form of biliousness, "which is really indigestion * * * from defective liver action, * * * which can be supplemented by the kidneys when they are very active, so that they throw out of the blood imperfectly oxidized nitrogenized waste very freely, * * * and thus some hours after a meal the urine is laden with lithates." In the same connection is the observation that albuminoid food alone is often digested more easily than when taken with hydrocarbons, a fact long ago pointed out by Sée, and by him made the basis of treatment for jaundice by means of an exclusively meat diet. Fothergill believes that hydrocarbons take up the oxygen from the blood and prevent nitrogenous substances from getting it. But it is generally admitted that no oxidations take place in the blood. The fact that the absorption of oxygen is increased by the amount of *fixed* albumen existing in the body, suggests a much more complex interpretation of the matter than that given by Dr. Fothergill.

There is a suggestion to use strychnine in jaundice in order to counteract the nervous depression caused by the bile salts circulating in the blood—a suggestion requiring the confirmation of experience. Another on the use of bromide of potassium to check diarrhœa of apparently reflex nervous origin, is theoretically more plausible, and is offered with the endorsement of experience.

This is really all we can find of value in these two chapters. If the poverty of suggestion depends on the want of space, it is provoking to find this taken up with the contra-indications of iron in gastric catarrh and menorrhagia; with the time-honored and dangerously unqualified advice to give castor oil in dysentery of children, in order to remove "offending masses," and with the remark that "it is scarcely possible to write dispassionately when we find calomel the constant visitor or even occupant of the nursery." "Calomel and gray powder are excellent in their place, but when improperly used they are very objectionable." Now the inexperienced practitioner, who alone should be expected to read a book of this kind, would like to know *what* is their place and *when* they are objectionable, but this is nowhere hinted at.

In the chapter on the respiratory system, the most useful suggestion is in regard to strychnine as a stimulant to the respiratory

centres: hence of value in suffocative bronchitis. This suggestion was already made in the writer's little book on antagonism. All the cases there quoted were in adults, and we believe there are theoretical grounds for thinking that the remedy would be of much less value in the capillary bronchitis of children.

It is always useful to remember gout, as being possibly at the bottom of chronic bronchitis, as well as of much other mischief. There is constant reference to gout in the book, involving much unnecessary repetition; but, on the whole, it is the most useful part of the volume. Much less useful is the advice, which we read with surprise, to give depressant diaphoretics, as antimonial wine in croup; and still more the statement that, "when laryngeal disease is about to asphyxiate the patient, tracheotomy not only, gives immediate relief, but commonly leads to the cure of the laryngeal disease. This it accomplishes by the rest it furnishes. That the rest is the curative agent is shown by the fact that, if the person upon whom tracheotomy has been performed is a talkative person, the repair is much slower than where more perfect rest is given by taciturnity." We will take for granted that the writer is speaking exclusively of tracheotomy in non-diphtheritic disease, and thus explain the remarkable optimism of this statement. But does he mean to imply that any one ever *talks* with a tube in his trachea?

As for assimilation and the digestive system, so for excretion and the kidneys, the two chapters, as Dr. Fothergill treats the subject, had much better have been combined. Throughout them we can only find the following remarks rising above the level of truisms. That so many physiological processes are known to be affected by ferments; that many others, as, for instance, the evolution of animal heat, are probably dependent upon as yet unknown ferments scattered throughout the body; and that all ferments are nitrogenized substances; and further, are really excrementitial. The not unfrequent necessity which exists for increasing waste, as a preliminary to attempts at increasing assimilation; the anæmiating effect upon anæmic people of the obstinate constipation to which they are so liable, are also matters not very recondite, but worthy of a little emphasis. In the immediate neighborhood is much to render the reader impatient. As an example we would quote some really antiquated pages upon "alteratives."

"These," says our author, "are a class of agents too little understood by the merely well-taught hospital student, but with

which he will do well to make himself more familiar, if he desires success in private practice. Their effect not being explicable or demonstrable by actual experiment (?) they are too often neglected, to the injury both of the medical man and patient. They were much more used by the practitioner of the last generation than they are now; but we are not quite certain that we are better practitioners than our predecessors. *Their* (?) neglect is one of the surest pieces of evidence that while our profession as a science (?) has distinctly improved, in some respects it has retrograded as an art."

In trying to arrest this retrograde movement, Dr. Fothergill classes together arsenic and mercury, as being both remedies which increase waste, and are therefore useful to accelerate excretion. The well-known fact is, that arsenic *diminishes* waste, diminishes the elimination of carbonic acid and of urea, increases fat, and, in its ultimate molecular action, is, in therapeutic doses, the direct opposite of mercury. In recommending mercury, again, the author entirely overlooks any benefit to be derived from its long continuance,—except in syphilis; overlooks, therefore, the utility of the bichloride in chronic inflammations; lays stress exclusively upon "occasional" purgations by calomel; an indication that may often be really much better fulfilled by salines or other remedies.

The chapter on inflammation is unsatisfactory from beginning to end. The physiological prolegomena in it are so scanty and imperfect, and have so little to do with the traditional directions given for treatment, that they had much better have been omitted altogether. Almost the same might be said of the chapter on fever. Here, indeed, the fundamental conception of a dual cause, namely, increased production, and diminished elimination of heat, is at first clearly stated, but the statement is immediately afterward obscured. We should imagine that Dr. Fothergill had never been obliged to combat anything more serious than an ephemera, for he places mineral acids at the head of antipyretics; is scanty about quinine, digitalis, or veratrum; evidently has no personal experience with cold; and just mentions salicylic acid in a note as he is going through the press. We cannot avoid noticing—with some horror—that nitrate of silver cauterizations are advised in diphtheria.

One other piece of therapeusis, or rather therapeutic theory, calls for notice: namely, the classification of all remedies for "diseases peculiar to women," under the title "anaphrodisiacs."

The pathogenetic theory implied by this title, is carefully enunciated by the author in his researches on menorrhagia and leucorrhœa. Local treatment of such disorders is just hinted at and immediately referred to the "obstetric physician." The chapter on the nervous system calls for no comment. The bulk of it consists of a little essay on sleeplessness, formerly written by the author, and now reprinted at the "expressed wish of several (complaisant) friends."

The chapter that has the nearest approach to originality, is that on action and inaction, wherein a few good remarks are made on the advantage of combining antagonistic medicines. We believe this method has indeed a large future in therapeutics.

In summoning up our impressions of a book whose popularity has already demanded a second edition, we should say that, after a slight glance at its pages, we should have been inclined to regard it as quite interesting, but that the more we read in it, the less we liked it; and, after a minute examination, we felt inclined to dislike it very much. As a guide for the inexperienced, it is really dangerous from its incompleteness and its plentiful lack of that "rationale" which it promises at the outset to provide. It belongs, moreover, to a class of books which steadily tend to weaken the mental powers of those who read much of them, and to slacken efforts toward "much understanding of the nature of things, including the nature of disease." For any one already familiar with the ground traversed by the author, and able to read him critically, the book may be recommended for an evening's leisure, while the memorandum book records, for even easier reference, the occasional hint or prescription which may be found of service. [M. P. J.]

On the Bile, Jaundice, and Bilious Diseases. By J. WICKHAM LEGG, Fellow of the Royal College of Physicians, London, Assistant Physician to St. Bartholemew's Hospital, etc. 8vo, pp. 719, New York: D. Appleton & Co., 1880.

We are glad to see that Dr. Legg has resisted the temptation—if he ever felt it—of writing a "practical treatise" on jaundice. The time has come for a full discussion of the numerous questions involved in this symptom, and our author, it seems to us, has wisely sacrificed the immediate popularity of his work for the more important object of presenting his subject with the fulness shown in the bulky volume before us. The old Greek proverb that "a great book is a great evil," still passes current with that large proportion of the medical profession which is in-

terested only in positive results, and cares but little for theoretical discussions which reach no settled conclusions, or for the historical treatment of disputed questions in physiology and pathology. Indeed, one of our own writers—himself not the least prolific and diffuse of contributors to medical literature—assures us that when the problem of the universe is finally solved, the solution will be presented within the limits of a score or two of pages. Perhaps it will be, but we fear this devoutly to-be-wished-for consummation will be reached only in the very remote future. In the meantime there remain several less complex problems which obstinately refuse to be settled, and have to be attempted by tedious methods. In difficult and prolonged investigations, like the present inquiry, the very history of these attempts and failures should act as a stimulus rather than as a discouragement to further effort, and instead of reproaching Dr. Legg with the barren results which fill this volume, we should congratulate ourselves that he has had the courage to place the subject before us in all its incompleteness.

The chemistry of the bile seems to be in a fairly satisfactory condition, but the same cannot be said of its physiology, an accurate knowledge of which must necessarily precede a clear conception of the pathological relations of the fluid. Here our information is extremely defective. Whether the bile is an excrement, or a secretion, or partakes of the nature of both; what part the bile plays in intestinal digestion and absorption; into what elements it is decomposed before or after its passage into the blood from the intestinal canal; and what are the final functions and destiny of these products of decomposition,—upon these points various plausible and even probable conjectures have been attempted, but we can hardly be said to have reached any demonstrated conclusions.

In this state of uncertainty with respect to the physiology of the bile, it is not surprising that its pathology should be involved in still greater obscurity. The present volume discusses controverted points with unusual fulness, and, in the main, with fairness and good judgment, so that our criticism will be confined to a few details. Dr. Legg admits only a single cause of jaundice, viz.: *absorption of bile*, and says that "among the theories of jaundice it is the only one grounded upon observation and experiment, and the only one to which universal assent may be demanded." This view has certainly the merit of simplicity, but we doubt whether it can be sustained. Recent experiments, it is

true, have shown that absorption of bile in the liver may take place much more readily than was formerly supposed, and that there is, therefore, less necessity for resorting to other theories in obscure forms of jaundice. Thus, absorption may be produced by a very slight obstruction to the escape of bile, such as a plug of mucus or even a moderate infiltration of the mucous membrane of the common duct, or of the mucosa of the duodenum at the orifice of the duct; and the same result may occur, as Heidenhain has shown experimentally, simply in consequence of lowered pressure in the blood-vessels of the liver—a condition which favors the passage of bile into the blood rather than into the capillary bile ducts. This much must be granted, but we cannot agree with Dr. Legg that the doctrine of hæmatogenous jaundice is to be rejected absolutely. This theory turns, chiefly, upon the identity between the pigments of the bile and blood. If bilirubin and hæmatoidin can be proved to be radically dissimilar bodies the theory must fall to the ground. Such a discovery our author claims has been made by Holm, Städeler and others. The following differential table* certainly presents a striking contrast :

Bilirubin.	Holm's Hæmatoidin.
1. Acts as a weak acid.	1. An indifferent body.
2. Dissolves in carbon disulphide with a golden yellow color.	2. Dissolves in carbon disulphide with a bright red color, and in dilute solution with an orange red.
3. Insoluble in ether.	3. Very soluble in ether.
4. Very soluble in alkalies.	4. Insoluble in alkalies.
5. Responds to Gmelin's test.	5. Is merely discolored by Gmelin's test.
6. Is separated from the chloroform-solution.	6. Is not separated from the chloroform-solution.

Heidenhain† has shown, however, that the discrepancies between Holm's results and those of other observers who have confirmed Virchow's original theory as to the identity of the two pigments, may be accounted for by the fact that the yellow body, which Holm obtained from the corpora lutea of the cow, is not true hæmatoidin. "To be sure," he says, "hæmatoidin crystals are often found in the corpora lutea. But if corpora lutea be treated indiscriminately with chloroform, as Holm has done, a pigment-solution is obtained, which behaves quite differently from

* Hermann's Handbuch der Physiologie, 1880, Bd. v, p. 246.

† Ibidem.

the hæmatoidin derived from cerebral apoplexies, etc. That the latter shares all the properties of bilirubin I am entirely satisfied from the treatment with chloroform of masses in the cerebral cortex which contained large amounts of hæmatoidin crystals, and I am confident also that there is scarcely anything common to hæmatoidin and the pigment obtained by Holm's method from the corpora lutea of the cow, except a certain resemblance in the color of the chloroform-solution. The latter, after a few minutes' exposure to sunlight, becomes green, if it contains bilirubin or hæmatoidin, while if it contains Holm's substance it long remains unchanged, and only gradually fades in color without turning green. The distinctions which Holm draws between his supposed hæmatoidin and bilirubin apply equally to the former pigment and true hæmatoidin from cerebral extravasations."

The question of hæmatogenous jaundice, then, can hardly be regarded as closed, so far as this portion of the argument is concerned. Dr. Legg claims that the theory has made but little progress in France and England, but he fails to note that it is very generally accepted and still maintains its ground in Germany.

The suppression theory of jaundice, although it must likewise stand or fall with the essential identity of the two pigments, rests in other respects upon a different basis of evidence. The advocates of hæmatogenous jaundice are not necessarily committed to its support, though they may be disposed to regard it more favorably than Dr. Legg. For while it is quite possible that certain blood-poisons may set free the pigment of the blood from its loose combination with hæmoglobin in consequence of dissolution of the corpuscles, it by no means follows that such a separation results from simple suppression of bile. The fact that the coloring matter of the bile does not accumulate in the blood when the liver is extirpated in cold-blooded animals (frogs) must certainly be regarded as a very strong argument against this theory.

The most unsatisfactory chapter in the book, to our mind, is the final one on "Bilious Diseases." Dr. Legg is, no doubt, quite right in ascribing most of the symptoms of "biliousness" to gastro-intestinal catarrh, and we can hardly blame him for bluntly stating that modern science can detect no other morbid conditions produced by biliary disorders than jaundice and gall-stones. This is a mortifying confession, notwithstanding, and we should like to see the iconoclastic spirit manifested in this chapter tempered with a little more humility. If we are not much mistaken, the

future will show a reaction against this ridicule of ancient doctrines ; and functional derangements of the liver, other than those accompanying jaundice, will play a much more important part in pathology than would seem possible after perusal of this article.

A word more of adverse criticism and we have done with this ungrateful portion of our task. Throughout the work Dr. Legg has used the word *prodroma* as a *plural* noun in the sense of "premonitory symptoms." This can hardly be a typographical error for *prodromata*, the word generally employed in this sense, as *prodroma* is used at least a score of times in connection with a plural verb. If this usage were intentional we are at a loss to know what precedent he has for it. Admitting that "prodromata" is incorrect—as it undoubtedly is, for there is no such Latin or Greek word as "prodroma" in the singular number—we are unwilling to accept the word he has used, although it may be somewhat more correct. If "prodroma" be plural, what word would he use for the singular? "Prodroma," plural, may possibly be employed as a noun for the neuter plural of the adjective *Πρόδρομος, ον*, "running before, etc," but this would necessitate the use of "prodromon" in the singular to express "premonitory symptom." However correct this might be, it is certainly inelegant, and is, moreover, unnecessary, for the word "probrome" from the Greek noun *Προ'δρομος*, Latin prodromus, has long been used in French medical writings, and is often employed in the plural, "prodromes," by English writers. "Prodromata" will have to be abandoned, as it is a spurious word, but we should be sorry to see its place taken by Dr. Legg's substitute for it. "Prodrome" and "prodromes" have good English terminations, and are in every way unobjectionable.*

As we have already intimated, not the least valuable portion of the present work is that which deals with the historical treatment of controverted points. We know of no other treatise in which this part of the subject is dealt with so fully or satisfactorily. The same thoroughness which characterized Dr. Legg's excellent monograph on hæmophilia is very noticeable here. The work is a mine of historical information, and if it had no other merit than this it would still take high rank as an authority. The author has, however, contributed a considerable amount of original work,

*We have referred these points to Mr. Charlton T. Lewis, of New York, whose authority upon such questions will be generally recognized, and have received from him a note confirming the views above expressed.

especially results of experimental investigation with respect to the functions of the liver in jaundice (Chap. xiv).

In conclusion, we again extend a cordial welcome to this important contribution to medical literature. [A. B. B.]

A Practical Treatise on Tumors of the Mammary Gland, by SAMUEL W. GROSS, pp. 241. New York, D. Appleton & Co., 1880.

One feels a great diffidence in undertaking a critical examination of a theme which has not, as the author informs us in his preface, "up to the present time constituted the subject of a systematic and strictly accurate treatise," especially when the author of the theme has "studied and investigated" it with the expressed purpose "to fill this void." Exactly what the author may have in view when he speaks of a "strictly accurate treatise" we are not able to determine. Such an achievement—if connected with a medical or surgical subject, particularly one involved in almost traditional obscurity, like the accurate nature of tumors—is a consummation much to be desired. We do not wish or intend to assume for one moment that because the author, in the present instance, has written what he no doubt believes to be, and what we may be willing to concede to be, the best treatise on mammary tumors extant, that it is necessarily or probably a "strictly accurate treatise."

The classification which he employs is a good one and seems to possess the philosophical application ascribed to it. We are glad of the opportunity to commend any scientific method which, while it simplifies a subject, opens up a new field for thought. The classification of this subject to be found in the average text-books cannot of necessity be very elaborate; we have long thought that something like a unity of action on the part of authors regarding the nomenclature of tumors would add much to the happiness and understanding of the reader; in fact, it has long seemed as if every individual who has engaged in this subject felt it to be a duty, which he was unwilling or unable to shirk, to furnish a new classification.

In the present instance we find but little deviation from the custom except, perhaps, in the introduction of an unusually simple and concise arrangement.

It is to be regretted that the older and more practical classification of tumors into benign, malignant, etc., must give way to one

based on the closer discrimination which results from scientific observation. It will be difficult, for some time to come, at least, to satisfy the practical surgeon with a classification of tumors based on their anatomical characteristics alone.

The evolutionary theories of mammary tumors are given a fair and candid consideration, especially the somewhat æsthetic one of Creighton.

The chapter on Etiology contains many important facts bearing on their causation, the majority of which possess the merit of being substantiated by figures.

The chapter on the General Anatomy of tumors contains but a few illustrations; however, these are chiefly original, and aptly exemplify the statement contained in the text.

The chapter on Fibroma we can commend as a fair epitome of that section of the work.

The chapters on Sarcoma and Carcinoma are the most extensive, and to our mind give to the book its greatest worth. The babel existing in the minds of the majority of medical men regarding the nature of sarcomatous growths when compared with carcinomatous, is very great. This little volume serves a double purpose; it not only treats graphically of these varieties in the mammary gland, but at the same time draws a clean-cut anatomical distinction between them. The statistical deductions are of extreme interest, especially those bearing on the great importance of cutting out carcinomatous growths. On page 169 can be found the following very satisfactory conclusions regarding the surgical treatment of these tumors.

First.—"When left to itself, carcinoma inevitably kills by its baneful consequence as a local disease, or by its remote multiplication." This state of affairs has been recognized by the profession for a long time; therefore the statement constitutes no addition to the subject, but its importance as a statement becomes markedly manifest, as the author, no doubt, intended it should, when one comes to consider it in relation to the subsequent conclusions.

Second.—"That one in six, or 16.77 per cent. of the patients die of the operation itself;" but he proves this risk admissible, since it "adds twelve months to the life of the patient."

Third.—"That thorough operation definitely cures 9.05 per cent., or more than half as many as it destroys."

Fourth.—"That patients are safe from reproduction after three years have elapsed."

Finally.—"Recurrence may be delayed or prevented by *cleaning out the axilla,** at the same time the breast is removed. It has long been the practice to remove from the axilla all the infected glands.

The author recommends (page 169) "cleaning out the cavity in all operations," leading one, at first, to surmise that the final conclusion is based upon this having been done, which, of course, cannot be true of each of the large number of cases he had under consideration.

The final conclusion is based, no doubt, upon the belief of the author that if the axilla had been cleaned out in all the previous operations the success attained would have been much more satisfactory ; however, any one who possesses data sufficient to sustain such a number of important conclusions as some of these just enumerated, is clearly entitled to the thanks of his professional brethren, as well as the gratitude of their patients. The chapter on Adenoma should be read by all who wish to see the nature of this growth well defined.

The chapters on Myxoma and Cysts, while not extensive, are eminently instructive. Under the heading of "Diagnosis of tumors of the mammary gland" is found a tabulated statement of the "affinities and contrasts" of carcinomatous and non-carcinomatous growths. This will prove of indispensable value as a ready-reckoner of their diagnostic points. On page 208 is to be found a similar arrangement applicable to the diagnosis of six solid from cystic forms of non-carcinomatous disease of the gland. Under the head of "Treatment" we find, what one would expect after reading the conclusions arrived at by the author, the early and complete removal of all questionable growths, together with a cleaning out of the axilla.

The appearance of the volume is good. The diction of the author is inclined occasionally to be extravagant, due, no doubt, to his earnest nature and correct belief that what he has to say upon the subject is entitled to close attention. An occasional error in syntax and orthography is noticeable—not of sufficient importance, however, to mar the excellence of the work.

In conclusion, we can sincerely congratulate Dr. Gross on having contributed an important work to the profession, written upon a subject which has heretofore received less than its share of the professional attention which its importance demands. [J. D. B.]

* Italics are our own.

ORIGINAL OBSERVATIONS.

ON HEREDITY IN PROGRESSIVE MUSCULAR ATROPHY AS ILLUSTATED IN THE FARR FAMILY OF VERMONT.

By WILLIAM OSLER, M. D., M. R. C. P., Lond.

PROFESSOR OF THE INSTITUTES OF MEDICINE IN MC GILL UNIVERSITY, MONTREAL.

The accompanying genealogical chart of the Farr family illustrates well the hereditary nature of progressive muscular atrophy.

I will first give a brief account of the member of the family who has been under my care : Erastus Farr, aged 47, a farmer, from Vermont State, admitted to General Hospital September 16, 1880, complaining of weakness in the left leg and peculiar twitchings in the muscles of various parts of the body. He is a tall, large-boned man of medium muscular development.

History.—Has been a hard worker, very temperate, never had any serious illness. Is married, has seven children, all of whom are well. About fourteen months ago began to notice twitchings of the muscles of the left buttock and thigh, which gradually increased in frequency, and within six months after their onset he felt the left leg weaker than the right. Has had no pain, only the uneasy sensations caused by the muscular tremors, which he describes as occasionally accompanied by a feeling of nausea. During this year the left leg has got steadily weaker and has diminished considerably in size. The twitchings have also become general and occur irregularly in different muscles.

Present condition.—When stripped, the left leg is seen to be smaller than the right, owing to uniform wasting of the muscles. Measurement gives a difference of 2.5 cent. in the circumference of the calves, and 7 cent. in that of the thighs in the middle third. The atrophy is best marked in the hamstring and gluteal muscles,

and extends slightly to those of the lumbar region of the same side. Fibrillar twitchings are of frequent occurrence in the muscles of the affected leg, and also in those of the trunk and other extremities. The strength of the left leg is greatly reduced. Sensation is less acute than normal in the legs; the points of the æsthesiometer have to be separated over 7 cent. before two impressions are perceived, and there is scarcely any difference in this respect between the legs or different parts of them. The electro-contractility of the muscles is preserved. In walking, patient requires the aid of a stick, and drags the left leg very much. He remained in hospital about a month, and was treated with the galvanic and faradic currents without evident benefit, though he thought himself somewhat improved.

Family history.—Thirteen individuals in two generations have been affected, nine of whom have died.

The following is a brief record of the cases:

Samuel Farr, father of patient, died at age of 61; ill over two years. Patient cannot say what his paternal grandfather died of; never heard that it existed in that generation.

Samuel Farr had five brothers and sisters, two of whom were affected. One brother,

Erastus, who died at the age of 40. This was the first case heard of in the family. One sister,

Mrs. Streeter, who died at the age of 54.

It is probable also that another sister, Mrs. Stoddart, had the disease. She died of paralysis, but whether this form or not is doubtful.

Ten members of the second generation have been affected. Two of the patient's brothers and one sister:

Samuel, who died at the age of 45; ill over two years. Had six children.

Wesley, aged 41, at present affected. Has no evident wasting, but the fibrillar twitchings have begun, and he has rheumatic pains. Has two children.

Ellen, died at the age of 27. Had four children.

Six of the patient's cousins, as follows:

Almira (daughter of Mrs. Stoddart), aged 45, still living, has been ill over two years. Has two children, one a cripple with legs undeveloped.

Hiram, son of Erastus, died at the age of 45. Two children living, one 30 years old.

Four children of Mrs. Streeter:

Mrs. Alexander, died at age of 55. Four children living.

Mrs. Robinson, died at age of 46. Three children.

Mrs. Alexander, aged 48, still living, arms much affected; cannot lift them.

Hiram, died at age of 24; ill several years; disease began in the legs.

Thus, of the 13 members of the family affected, 6 were females and 7 males, a larger proportion of the former than is common in this disease.

With the exception of two, all of the cases occurred, or proved fatal, above the age of 40. Of the 10 instances in the second generation, 5 are the offspring of males (Erastus and Samuel), and 5 the offspring of females (Mrs. Streeter and Mrs. Stoddart). The disease has not yet appeared in the third generation, which promises between 40 and 50 individuals, several of whom are over 30 years of age.

I append a genealogical table of this family, in order to show its liability to progressive muscular atrophy, and also reproduce Prof. Naunyn's table of the Bessel family. (*Berliner Med. Wochenschrift*, Nos. 42 and 43, 1873.)

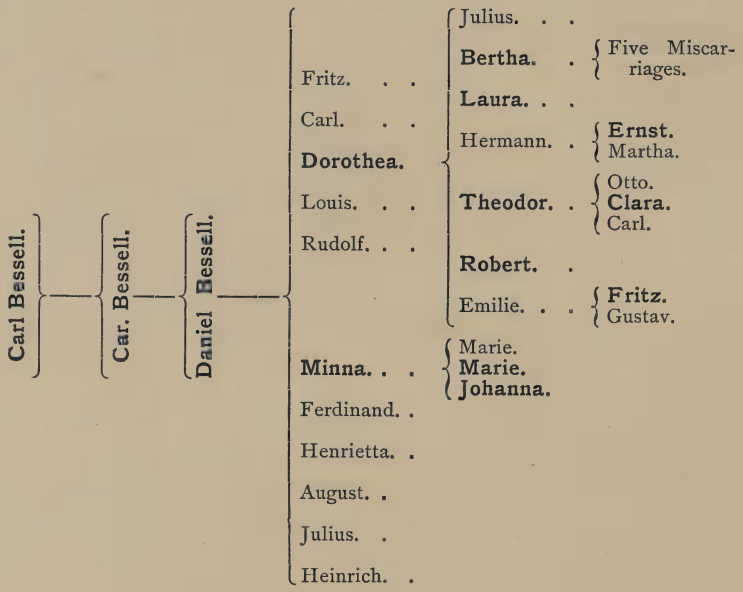
GENEALOGY OF THE FARR FAMILY.

SAMUEL FARR.

- Rossin Farr. . . . . Died at 50. — 5 Children. . . . . No disease.
- Russell Farr. . . . . Died at 60. Drinker. — 5 Children. . . . . No disease.
- **Samuel Farr.** . . . . Died at 61. Ill two years.
  - **Samuel.** 45. . . . . 6 Children.
  - **Ellen.** 27. . . . . 4 Children.
  - Maria. . . . . .
  - Rossil. . . . . .
  - **Wesley.** 41. . . . . 2 Children.
  - **Erastus.** 47. . . . . 8 Children.
  - Edwin. . . . . .
  - Matilda. . . . . .
- **Erastus Farr.** . . . . Died at 40. First case heard of in Family.
  - 3 Children died of scarlet fever.
  - **Hiram.** 55. . . . . 2 Children.
  - Orary. . . . . .
- Mrs. Stoddart. ? . . . Died at 40.
  - Altina. 35. . . . . 3 Children.
  - William. died 24. . . .
  - Adaline. . . . . 1 Child.
  - **Almira.** 55. . . . . 2 Children. 1 child legs undeveloped.
  - Almond. 60. . . . . 3 Children.
- **Mrs. Streeter.** . . . Died at 54.
  - Joil. . . . . .
  - **Hiram.** 24. . . . . .
  - **Mrs. Alexander.** 55. 4 Children. All well.
  - **Mrs. Robinson.** 46. 3 Children. All well.
  - **Mrs. Alexander.** 48.
  - Mrs. Smith. 38. . . . .
  - Mrs. Cleveland. 60. . . 3 Children. All well.

The individuals whose names are printed in heavy face type were the subjects of the disease—the others escaped.

GENEALOGY OF THE BESSEL FAMILY (NAUNYN).



The individuals whose names are printed in heavy-faced type were the subjects of the disease—the others escaped.

WELLCOME LIBRARY INSTITUTE